Field Experiments in Labor Economics

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Zurich, April 14th 2010
The Dean: Prof. Dr. Dr. Josef Falkinger

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Introduction

Economics was long considered a non-experimental science that mainly relied on the correlational analysis of real-world economies. Only twenty-five years ago, Samuelson and Nordhaus (1985) claimed that:

> One possible way of figuring out economic laws [...] is by controlled experiments. [...] Economists (unfortunately) [...] cannot perform the controlled experiments of chemists or biologists because they cannot easily control other important factors. Like astronomers or meteorologists, they generally must be content largely to observe.

In the last two decades, however, the use of experimental methods in economics has grown steadily and radically challenged this view. The conferral of the 2002 Nobel Prize on Vernon Smith and the growing number of publications in top journals demonstrate the increasing importance of experimental economics.

Most economic experiments are computerized laboratory experiments. In the lab, the experimenter knows and controls every aspect of the decision environment. This tight control provides the experimenter with the possibility of easily implementing exogenous and randomized variations in environmental factors. Controlled variation of conditions allows for causal inference, which is essential for testing economic theories. A key question, however, is whether empirical results from laboratory experiments can be generalized to the field (Levitt and List, 2007; Falk and Heckman, 2009). Skeptics of lab experiments criticize the stylized decision environments and the fact that subjects know they are participating in an experiment.

In response to these criticisms, field experiments became quite popular as a bridge between laboratory and naturally-occurring field data.[1] The major advantage of field experiments derives from the possibility of observing subjects in a more natural, but still controlled environment. The purpose of an experiment determines whether enhanced realism is important. If the purpose extends beyond testing economic theories, field experiments clearly allow for a better understanding of the reality and thus have greater relevance to policy. Another advantage of field experiments is that subjects are unaware they are participants in an experiment and consequently do not know that the experimenter is observing their behavior. The subjects may still perceive that others observe them, but they cannot deliberately behave in a manner that the experimenter will view favorably. This feature prevents experimenter demand effects; these may confound the empirical results (Zizzo, forthcoming).

Field experiments, however, also entail several disadvantages when compared to laboratory experiments. The main disadvantage is that field experiments do not allow for the same degree of control over the environment as do lab experiments. This sometimes makes it difficult to distinguish between alternative theories. Another disadvantage of field experiments is the difficulty in replicating the empirical results with the collection of new data from a comparable environment. This difficulty arises because many field experiments require the cooperation of outside entities, such as firm or government representatives. Precise replication, however, is of great importance because it not only tests whether the results can be verified independently, but it also provides the experimenter with incentives to collect data carefully. The advantages and disadvantages of field compared to laboratory experiments underscore the complementarity of these methods in improving the state of knowledge in economics.

This thesis demonstrates with three examples of how field experiments can be implemented in the context of labor economics. Chapter 2 presents evidence from a field experiment that examines whether workers reciprocate wage increases with higher effort and the extent to which their response is related to their fairness perceptions and preferences. Economists traditionally assume that workers only respond to economic incentives.

[1]The word "field" in field experiments was coined in the pioneering experiments of Fisher (1926) who examined the impact of field conditions on agricultural yields. The first wave of economic field experiments started with the Hawthorne experiments in the 1920s. Their objective was to test whether better lightning conditions increase worker productivity (Mayo, 1933; Roethlisberger et al., 1939). Ross initiated the second wave in the 1960s, when she conducted a large-scale social experiment to explore the effects of negative income taxes on various outcomes, such as labor supply and consumption (Greenberg and Shroder, 2004).

However, some economists have long recognized that, in addition to economic incentives, workers are also strongly motivated by fairness concerns (Slichter, 1920; Marshall, 1925; Hicks, 1932). A prominent example is the fair wage-effort model by Akerlof and Yellen (1990). According to this hypothesis, workers feel entitled to a fair wage. The model assumes that if the actual wage is less than the fair wage, workers perceive higher wages as fairer and will consequently exert more effort. Extensive evidence from laboratory experiments supports a positive relationship between wages and effort. However, evidence that higher wages are perceived as fairer and consequently elicit higher effort in real-life work environments is less clear-cut. In our field experiment, we implemented wages increases and measured the associated changes in work performance. We later conducted a follow-up survey among the workers where we both determined the wage workers thought would be appropriate for their work and experimentally elicited their inclination for reciprocal fairness.

While Chapter 2 focuses on the positive relationship between wages and effort, Chapter 3 explores the determinants of the fair wage. This chapter describes the empirical results of a field experiment that tests whether workers reciprocate wage cuts with lower effort and the extent to which their response is influenced by the wages their coworkers earn. These questions are difficult to answer without experimental data because wage cuts, especially those aimed only at part of the workforce, are rarely observed in the field. According to Bewley (1999), firms often seem reluctant to cut wages because of the fear that wage cuts may inhibit work performance. Nevertheless, if wages are actually downwardly rigid, this has important implications for the functioning of labor markets, such as involuntary unemployment or wage compression. In our field experiment, we allocated workers in teams of two and exposed either one or both team members to a wage cut.

Chapter 4 comprises the findings of a field experiment that tests the effects of social interaction on work performance in the presence of either positive or negative effort externalities. Many jobs involve effort externalities that create inefficiencies for the workers if they fail to internalize them. Substantial evidence from laboratory experiments shows that communication increases cooperation when positive externalities are in place. However, the role of social interaction in facilitating the internalization of both positive and negative externalities, as well as the limitations of this role remains poorly understood. In our field experiment, workers were invited in groups of four and they were randomly divided in teams of two. Team members either worked in the same or in a different office. Social interaction between team members was thus only possible when assigned to a

shared office. In addition, workers faced either team or relative incentives. Consequently, effort either imposed a positive or a negative externality on the team member's income. The results of this field experiment underscore the relevance of office arrangements for the design of optimal incentive schemes.

Overall, the findings presented in this thesis substantiate the value of field experiments as a method to enhance our understanding of labor markets.

Chapter 1

Fair Wages and Effort

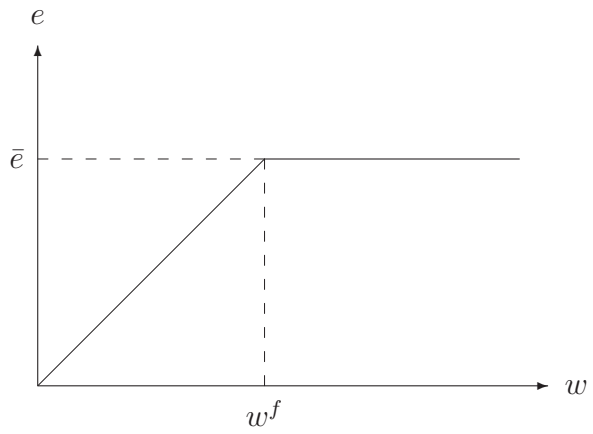
Chapter Summary

We conducted a field experiment to test whether wage increases induce workers to provide more work effort voluntarily. The experiment took place when a publisher hired workers to distribute a newly launched free newspaper at train stations and other public places. Workers were paid a flat wage and knew they did not have the prospect of long-term employment. Consequently, they had no economic incentives to exert more than minimal effort. We implemented wage increases and measured the associated changes in work performance. We also conducted a follow-up survey among the workers where we measured the wage workers thought would be appropriate for their work as well as workers' propensity for reciprocal fairness. We show that a higher wage significantly increased work performance. Further analysis shows that only workers who both considered the base wage to be unfairly low and who revealed fairness preferences drove this effect. This evidence strongly corroborates the fair-wage effort hypothesis, which has sweeping implications for the functioning of labor markets.

1.1 Introduction

Do fairness concerns drive worker behavior? There is a growing theoretical literature that stresses the importance of fairness and its far-reaching implications in the labor market.[1] A prominent example is the fair wage-effort hypothesis by Akerlof and Yellen (1990).[2] The model assumes that workers feel entitled to a fair wage. Accordingly, wage increases up to the fair wage lead to higher work effort. In contrast, pay raises above the fair wage do not affect effort. The basic idea of this model is that only wage increases that improve the perceived fairness of the wage are associated with higher effort (see Figure 1.1).

Figure 1.1: Fair Wage-Effort Hypothesis



The figure shows the behavioral assumption of the fair wage-effort hypothesis that can explain the existence of involuntary unemployment. If e denotes effort supplied, $\bar{e}$ is the upper bound of effort, w is the actual wage, and w^f is the fair wage, the fair wage-effort hypothesis says that $e = \bar{e} \cdot \min[\frac{w}{w^f}, 1]$.

Extensive evidence from laboratory experiments and from manager interviews strongly supports the notion that higher wages induce workers to provide more effort.[3] Nevertheless, experimental evidence in real-life jobs is less clear-cut and the effects found are

[1] For empirical evidence of potential economic implications, see Dickens and Katz (1987) on inter-industry wage differentials, Clark and Oswald (1994) on involuntary unemployment, and Bewley (1999) on downward wage rigidity.

[2] For a more recent formalization of a very similar idea, see Benjamin (2006), Danthine and Kurmann (2008), and Cabrales et al. (2008).

[3] See Fehr et al. (1993), Fehr and Falk (1999), Hannan et al. (2002), Brandts et al. (2004), Charness et al. (2004) and Charness (2004) for lab evidence from gift exchange experiments. Kaufman (1984), Doeringer et al. (1971), Blinder and Choi (1990), Agell and Lundborg (1995), Bewley (1995), Levine (1993) and Campbell III and Kamlani (1997) stress the importance of fairness in determination of pay as described in manager interviews.

usually very small. In a recent study, Gneezy and List (2006) find a positive effect of a wage increase on work performance. However, the effect is temporary and dissipates after three hours.[4] Kube et al. (2010*a*), who use a design similar to that of Gneezy and List (2006), find no significant impact of a wage increase on work performance. Al-Ubaydli et al. (2008) use a setting where workers were concerned with their reputation. They find a positive and significant performance difference between workers who were paid a high wage and those paid a low wage. Most recently, Kube et al. (2010*b*) find a very large and significant effect of a non-monetary gift on work performance. These conflicting results are sometimes taken as evidence that results from laboratory experiments do not generalize to the field (Levitt and List, 2007).

This chapter reports evidence from a field experiment that explores whether higher wages motivate workers to provide more work effort voluntarily. We also present evidence showing that workers' response to higher wages depends on the level of their fair wage as well as their propensity for reciprocal fairness. Our field experiment took place during a newspaper promotion, where a publisher hired workers from a promotion agency. It was clear from the outset that the newspaper promotion would only last a few weeks. Workers therefore had no prospect of long-term employment, thus ruling out reputation incentives.[5] Workers' task was to distribute copies of a newly launched newspaper at train stations and other public places. The promotion agency paid the workers an hourly wage of CHF 22.[6] In collaboration with the publisher, we raised workers' hourly wage by CHF 5 in a randomized and controlled way. Ten weeks after the newspaper promotion, we conducted a follow-up survey among the workers that was seemingly unrelated to the promotion. The survey asked questions concerning part-time work. Most importantly, the survey prompted the workers to indicate the wage they considered to be fair for their work. The survey concluded with a social dilemma experiment providing us a measure of workers' fairness preferences.

This setup improves on earlier studies in several ways. Earlier studies provided the

[4] In a pioneering field experiment, Pritchard et al. (1972) manipulate workers' perception of the fairness of their pay, while paying them all the same wage. However, they find no significant performance difference between workers who were made to believe that their pay was generous and those who were made to believe that their pay was unfairly low.

[5] Alternative efficiency wage models can also explain a positive relationship between wages and effort (Shapiro and Stiglitz, 1984; Bull, 1987; Hart and Holmström, 1987; MacLeod and Malcomson, 1989). In contrast to the fair wage-effort hypothesis, these models assume an infinite employment duration where workers care about their reputation. A setting where reputation incentives are absent is crucial for distinguishing between the fair wage-effort hypothesis and the other models.

[6] CHF 1 corresponds to \$0.96 (situation January 2010).

important first step of randomizing wages in a real-life work environment (Gneezy and List, 2006; Al-Ubaydli et al., 2008; Kube et al., 2010*a*,*b*). As mentioned earlier, a common finding in these studies is that raising wages above the baseline level has only a small and insignificant effect on work performance. These studies, however, are limited by their very small sample sizes which involved a maximum of 30 subjects, and in most cases 10 or less per treatment. Moreover, subjects were typically observed once over a period of six hours. Because of the low number of observations, statistical power is low, making it hard to reject the null hypothesis of no effect.[7] We, in contrast, observed 196 workers who were on the job for an average of seven three-hour shifts. This allows us to test the fair wage-effort hypothesis with greater statistical power. As in earlier studies, we only find a small effect of raising the wage on work performance. Because of the larger number of observations in our study, however, we can reject the null hypothesis of no effect.

It may be wrong to conclude from these results, however, that fairness concerns are irrelevant for worker behavior. All of the previous studies offered a comparatively high base wage, making it difficult to improve the perceived fairness of the wage. Since the fair wage-effort model assumes that workers are only likely to work more if they currently feel underpaid, these fairness perceptions need to be measured. Moreover, in contrast to laboratory experiments, where the researcher can control all environmental factors, workers' conception of a fair wage in the field is less clear and may strongly differ between individuals. Thus, we measured workers' fair wage, allowing us to go beyond studying the overall effect of a wage increase on work performance. We indeed find strong heterogeneity in fairness perceptions. In our study, about half of the workers considered the base wage to be inadequately low, while the other half considered the base pay appropriate. When we relate workers' response to their fairness perceptions, we find that wage increases up to the fair wage had a large impact on work performance, while pay raises above the fair wage had no effect on work performance, as postulated by the theory. These findings explain why the overall effect in our study, and possibly in others as well, is rather small.

Evidence from laboratory experiments also shows that some individuals have strictly selfish preferences and do not react to higher wages, or even to fairness manipulations. Our measure of workers' preferences allows us to classify them as reciprocal and non-reciprocal. We show that only reciprocal workers exerted more work when the wage was raised towards what they thought was fair. Conversely, non-reciprocal workers did not at all react to wage increases, even when they considered the base wage to be unfairly low.

[7]This problem is exacerbated by the absence of a within-subjects design.

Overall, our results strongly support the fair wage-effort hypothesis and provide an explanation for the seemingly incompatible findings in laboratory and field experiments.

1.2 Economic Environment

A major publisher launched a free daily newspaper in May 2006, and hired a promotion agency in order to publicize the newspaper.

1.2.1 Promotion Agency

The core business of the promotion agency is the organization of sales promotions. It retains a large pool of part-time employees it can contact when the specific need arises. Workers can typically sign up for work shifts two weeks prior to a shift. When workers agree to take on at least some shifts, they receive information about the required attire, hours of work, and hourly wage. Workers' task usually consists of distributing samples.

1.2.2 Newspaper Promotion

The promotion agency assigned workers to train stations and other public places and asked them to distribute copies of the newly launched newspaper to passers-by. Before starting work, workers met a supervisor who informed them about the assigned location.[8] Workers were reminded to approach potential readers actively and also to retain the plastic straps from the newspaper bundles that were fully distributed in order to have an accurate count of the number of copies dispersed.[9] Workers then went to their assigned locations to distribute copies during three hours. At the end of a shift, workers returned to their supervisor to return the straps from the distributed newspaper bundles and to check out.[10]

It is noteworthy that several aspects of this work environment did not encourage the workers to exert high effort. First, workers earned a flat wage rather than a piece rate,

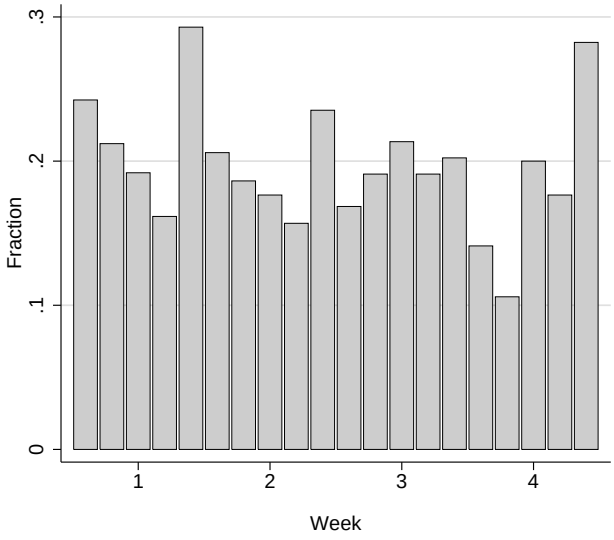
[8]Supervisors were full-time employees of the promotion agency and responsible for five to ten workers. Their task was to ensure that workers had a sufficient number of copies to distribute at all times. Supervisors were also instructed to make sure that no workers simply threw the newspaper away; the promotion agency has the policy of firing workers if they do so. No such incident was reported.

[9]Workers were also asked to keep an accurate count of the number of copies distributed from the last, partially distributed bundle.

[10]After a shift, supervisors went back to the locations to record the remaining number of copies and put them into the newspaper boxes. This procedure allowed us to verify the number of distributed copies at the worker level.

which meant that pay was independent of the number of copies distributed. Second, nobody could blame the workers if the newspaper demand was low and therefore the amount of copies distributed was low too. Third, the locations provided the workers with an attractive opportunity to shirk because they could easily hold private conversations in an unobtrusive way. Finally, the agency had a difficult time covering all locations due to the sheer size of the newspaper promotion. Consequently, about twenty percent of the planned work shifts could not be filled with workers, which meant that the locations remained unallocated on some days (see Figure 1.2). In summary, the threat of firing workers due to too little effort was not credible during the newspaper promotion.

Figure 1.2: Fraction of Unallocated Locations



1.3 Study

The study consists of two phases. First, we conducted a field experiment to analyze the impact of wage increases on work performance. Second, we conducted a follow-up survey to examine whether workers' response to the wage increases is related to their fairness perceptions and preferences.

1.3.1 Field Experiment

Our field experiment took place in the city of Zurich, Switzerland, and was conducted over a four-week period in June and July 2006. The promotion agency divided the city

into two equally-sized regions, based on the organizational structure of the newspaper promotion. Each region had its own manager who was in charge of the recruitment and assignment of workers and supervisors to the locations. Importantly, workers in one region were sometimes assigned to different locations in that region, but they were never assigned to work in the other region.

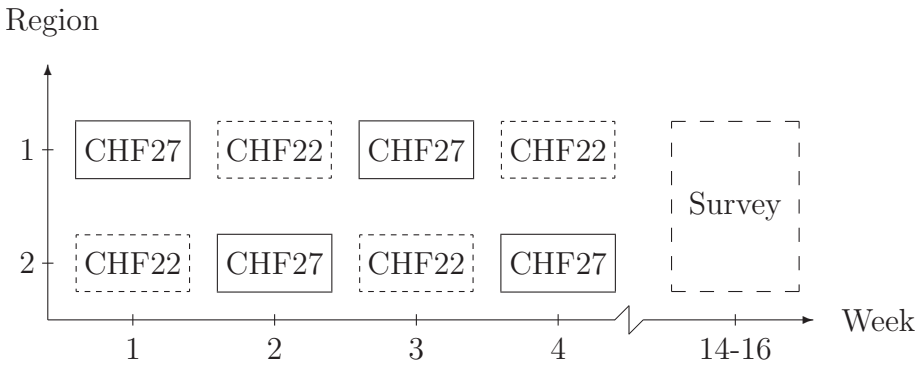
In the baseline treatment, workers were paid the hourly base wage of CHF 22, which we will refer to as "CHF22" condition. In the main treatment, the publisher raised workers' hourly wage by CHF 5, from CHF 22 to CHF 27. In the following, we will refer to this condition as "CHF27". Pay raises were implemented just before the beginning of a work shift. The publisher sent two announcements of the change: workers received a postcard as well as a text message on their mobile phones.[11] Together with the pay raise information, both the postcard and text message reminded the workers that it was important to retain the straps from the distributed newspaper bundles. In order to keep the treatment of workers constant, workers in the baseline treatment also received a postcard and a text message reminding them of the importance of keeping the straps from the distributed newspaper bundles.

Figure 1.3 presents the timing and change in treatments. In each of the four weeks, workers in one region were randomly assigned to treatment CHF27, while those of the other region served as a control and were assigned to treatment CHF22. There were two reasons for choosing a weekly rotation of the treatments. First, a weekly rotation, compared to a less frequent rotation, allows for a more robust identification of confounding time effects, which may have been large in view of the newspaper launching. Second, there was a strongly anticipated turnover after the second week of the experiment. Thus, a weekly rotation helped generate within-subjects variations in pay, enabling us to control for worker heterogeneity in the empirical analysis. A more frequent rotation, however, could have confused the supervisors who were in charge of distributing the correct postcards. Although treatment assignment changed every week, treatments were implemented on a day-to-day basis because workers could freely choose how many shifts they worked and they typically worked on an irregular basis. If asked, workers were told that the publisher decided when and where the high wage was payable, and that the promotion agency did not know this information in advance. This was done to rule out selection

[11]The postcard bore the publisher's logo and both the postcard and text message concluded with the name of the publisher. This was done in order to make it plain that the publisher, and not the promotion agency, was paying workers the wage supplement.

into work shifts where workers assumed that the high wage would be paid. Importantly, neither workers nor supervisors knew they were participating in an experiment.

Figure 1.3: Timing of Events



The figure visualizes the two phases of the study. First, workers of one region were randomly assigned to receive the high wage (treatment CHF27), while those of the other region received the base wage (treatment CHF22). Second, workers were asked to complete a survey on part-time work that included a social dilemma experiment.

Towards the end of the field experiment, the promotion agency sent a feedback form to the workers. Workers were asked questions concerning their work environment. In particular, the feedback form asked the workers to rate the fairness of the base and high wage on a five-point scale: "I consider the hourly base wage of CHF 22 for the exertion of this task to be [from *stingy* to *generous*]" and "I consider the hourly high wage of CHF 27 for the exertion of this task to be [from *stingy* to *generous*]". This serves as a manipulation check. In order to prevent confounding demand effects, workers were assured that their responses were anonymous.

1.3.2 Follow-Up Survey

In October 2006, we conducted a follow-up survey among the workers. The survey was mailed from the University of Zurich and informed the recipients that some firms had been asked to provide addresses from their part-time employees. The workers in the newspaper promotion had no indication that this survey was in any way connected to the variation in pay they had experienced earlier.

The survey asked a variety of questions related to part-time work and also collected demographic information. The recipients were asked to indicate up to three employers

from the previous six months, and they had to answer questions relating to each of their listed employers.[12] The questions of key interest to us were "How much (gross CHF per hour) did you earn at employer [*employer name*]?" and "How much (gross CHF per hour) do you think is appropriate for the exertion of this task at employer [*employer name*]?". These two questions allow us to identify workers' fairness perceptions and to relate them to their response to the wage increases.

At the end of the survey, workers were asked to participate in a paper and pencil experiment. The experiment was a one-shot, sequential social dilemma game with two players who were anonymously matched with a part-time employee from a different firm. Workers from the newspaper promotion were assigned to be second movers, and their decisions were elicited using the strategy method. The first movers had three options: they could keep CHF 6 for themselves and give CHF 18 to the second mover, keep CHF 12 and give CHF 12, or keep CHF 18 and give CHF 6. A second mover could reward or punish the first mover by assigning positive or negative points, respectively. Second movers could also decide not to assign any points at all. The reward and punishment technology was designed in such a way that one positive (negative) point cost the second mover CHF 1 and increased (decreased) the first mover's payoff by CHF 3. Second movers could assign up to two positive or negative points, respectively, per allocation. We use second mover decisions to classify the workers from the newspaper promotion as reciprocal or non-reciprocal. Second movers are classified as a reciprocal type if they assigned more positive points in (6,18) than in (12,12) or more positive points in (12,12) than in (18,6). Thus, in order to be counted as reciprocal type, second movers had to reward first movers for giving them a better allocation.

If the respondents fully completed and returned the survey within two weeks, they received a guaranteed amount of CHF 7 for completing the survey plus the amount earned in the social dilemma game.

1.3.3 Descriptive Statistics

Table 1.1 provides basic descriptive statistics of the experimental set-up. Workers, on average, handed out 230 copies per hour and worked 6.5 shifts over the four-week period of the field experiment. Of the 196 workers who participated in the field experiment, 113

[12]The six months covered the time period of the newspaper promotion.

workers answered the feedback form and 119 workers completed the follow-up survey.[13] Workers were relatively young (22 years) and mostly female (73 percent). Half of them were enrolled or already graduated from university (23 percent), respectively college (24 percent).

Table 1.1: Descriptive Statistics

Data Variable	Mean	Std. Dev.	Min.	Max.	N
Newspaper Promotion					
Hourly copies distributed	229.777	84.409	16.667	578.212	1269
# Work shifts	6.474	4.248	1	19	196
Feedback Form					
Fairness rating of high wage	2.858	0.844	1	5	113
Fairness rating of base wage	3.894	0.910	2	5	113
Follow-Up Survey					
Fair wage – base wage (in CHF/h)	1.097	2.056	–4	8	119
Age	22.465	4.698	16	42	114
Male	0.272	0.447	0	1	114
Foreigner	0.132	0.340	0	1	114
# Siblings	1.439	0.912	0	5	114
Secondary education	0.614	0.489	0	1	114
Apprenticeship	0.281	0.451	0	1	114
Continuing education	0.211	0.409	0	1	114
High school	0.675	0.470	0	1	114
College	0.237	0.427	0	1	114
University	0.228	0.421	0	1	114
Points if (6, 18)	0.890	0.885	–2	2	118
Points if (12, 12)	0.254	0.730	–2	2	118
Points if (18, 6)	–0.559	1.050	–2	2	118

The table describes the data used in this study. Data come from three types of sources: the newspaper promotion, covering the number of copies distributed per actual working hour and the number of work shifts taken, the feedback form, containing anonymous fairness judgments of the two wage levels, and the follow-up survey, which allows to track individual fairness perceptions, characteristics and preferences.

[13]Five survey participants did not answer the questions on personal characteristics and one worker did not participate in the social dilemma experiment.

1.3.4 Randomization Check

We now test whether individual characteristics were balanced across treatments using data from the follow-up survey. Table 1.2 reports the means and standard deviations of these characteristics for each treatment. The last column of this table presents the results of a non-parametric test for the null hypothesis of perfect randomization. It is clear that we cannot reject the null of no difference between treatments for any of the characteristics.[14]

Table 1.2: Randomization Check

	Treatment				
Data	CHF22		CHF27		
Variable	Mean	Std. Dev.	Mean	Std. Dev.	p-value
Follow-Up Survey					
Fair wage – base wage	1.088	(2.099)	1.081	(2.142)	0.694
Age	23.370	(5.257)	23.344	(5.397)	0.770
Male	0.281	(0.450)	0.267	(0.443)	0.681
Foreigner	0.161	(0.368)	0.172	(0.378)	0.697
# Siblings	1.376	(0.849)	1.367	(0.854)	0.912
Secondary education	0.648	(0.478)	0.633	(0.483)	0.692
Apprenticeship	0.331	(0.471)	0.308	(0.462)	0.516
Continuing education	0.248	(0.432)	0.242	(0.429)	0.852
High school	0.618	(0.487)	0.658	(0.475)	0.268
College	0.251	(0.434)	0.211	(0.409)	0.215
University	0.245	(0.431)	0.211	(0.409)	0.290
Points if (6, 18)	0.811	(0.902)	0.857	0.891	0.461
Points if (12, 12)	0.251	(0.726)	0.248	(0.684)	0.878
Points if (18, 6)	–0.651	(1.017)	–0.663	(1.004)	0.976

Treatment averages (and standard deviations in parentheses) are reported in the first four columns. The last column contains p-values (χ^2 tests for binary and Mann-Whitney tests for non-binary variables) for the null hypothesis of perfect randomization.

[14]Notice that the p-values for the tests are calculated assuming independence between all observations. Because we have repeated observations from individuals, this likely understates the variance in the data. Thus, if anything, these tests are biased toward finding a difference. Nevertheless, the lowest p-value we find is 0.215.

1.3.5 Check for Selection Bias

As the recruitment and assignment to locations was done approximately two weeks prior to a work shift, workers should not have been able to select into treatment CHF27. However, we examine this in two ways. First, we run the following regression

$$s_{ic} = \alpha_i + I(\text{CHF27})_{ic} + \epsilon_{ic}, \tag{1.1}$$

where the dependent variable s_{ic} is the number of shifts that worker i worked in treatment c. We include a fixed effect α_i for each worker and a treatment indicator for CHF27. Finally, ϵ_{ic} is the idiosyncratic error term, which we allow to be correlated within workers.

Second, we run another regression to check whether the fraction of unallocated work shifts differed by treatments. That is, we run the regression

$$u_{tc} = \delta_t + I(\text{CHF27})_{tc} + \epsilon_{ic}, \tag{1.2}$$

where the dependent variable u_{tc} is the ratio of unallocated shifts per day t and treatment c. We include day fixed effects δ_t in the estimation and allow the error term ϵ_{ic} to be correlated within days.

Column (1) of Table 1.3 shows the estimation results of equation (1.1). Workers, on average, worked 3.14 shifts in treatment CHF22, and 3.32 shifts in treatment CHF27 respectively. This difference is small and insignificant. Column (2) reveals the estimation results of equation (1.2). Again, the indicator for treatment CHF27 is close to zero and insignificant. Taken together, we find no indication that workers were able to select into the high-wage treatment.

1.4 Results

This section reports the results from our study presented in four steps. First, we reveal whether workers considered the base wage to be fair for this job. We also test whether the higher wage improved the perceived fairness of the wage. Second, we analyze whether workers reciprocated the higher wage with higher work performance. Third, we examine whether workers' response to wage increases was related to their fairness perceptions. Finally, we explore whether workers' preferences affected their response to the wage increases.

Table 1.3: Check for Selection Bias

Dependent variables: (1) # shifts, (2) fraction of unallocated shifts	(1)	(2)
CHF27 (=1)	0.189	0.008
	(0.184)	(0.028)
Constant	3.143***	0.193***
	(0.092)	(0.014)
Individual fixed effects	Yes	No
Day fixed effects	No	Yes
N	392	40
R^2	0.846	0.519

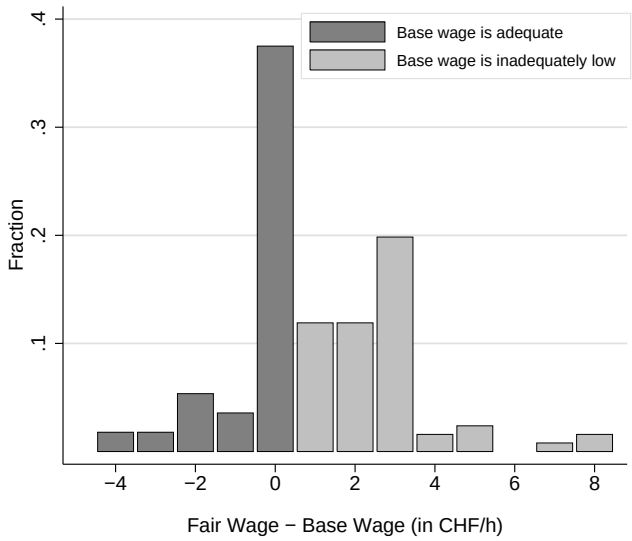
OLS estimates. The unit of observation in column (1) is worker i in treatment c, and the dependent variable is the number of shifts per treatment. The unit of observation in column (2) is day t in treatment c, and the dependent variable is the fraction of unfilled shifts. Fixed effects are normalized such that the constant reflects the mean of the omitted category. Standard errors, clustered by workers, respectively days, in parentheses. Level of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

1.4.1 Fairness Perceptions of the Wage

There is substantial heterogeneity in workers' fairness perceptions, as shown in Figure 1.4. Based on data from the follow-up survey, the figure displays the difference between the wage workers considered to be fair, and that they were effectively paid in the baseline treatment. Thus, a positive number indicates that workers felt underpaid, while a negative number indicates that they felt overpaid. The figure shows that 53 percent considered the base wage to be inadequately low. Thus, a large number of workers accepted the job even though they thought that the base wage was unfairly low. Of the remaining 47 percent, 35 percent perceived the base wage as adequate, while very few (12 percent) stated that they were paid more in the baseline treatment than they thought was adequate for this job.

Figure 1.4 suggests that an improvement in the perceived fairness of the wage was possible for a slight majority of workers. Using data from the anonymous feedback form, we indeed find strong evidence that the high wage significantly improved the perceived fairness of the wage, as Figure 1.5 indicates. The figure shows that 30 percent rated the base wage in the two lowest fairness categories, while only 2 percent reported the same for the high wage. Thus, there is a clear shift to a fairer evaluation of pay. In particular, the strongest shift in the distribution of fairness judgments seems to come from the bottom

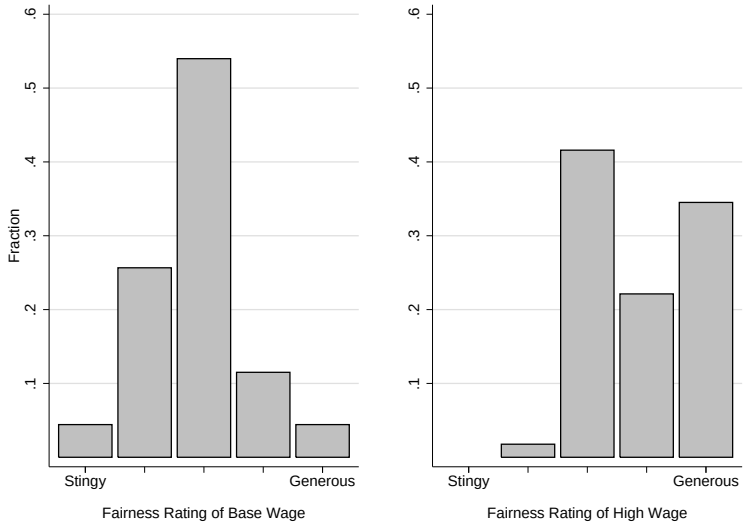
Figure 1.4: Fairness Perceptions of the Base Wage



The figure plots the difference between the wage workers considered to be fair for this job and the base wage (treatment CHF22).

end of the distribution. A Wilcoxon signed-rank test clearly rejects the null hypothesis that both distributions are the same ($p < 0.001$). This result confirms that we effectively manipulated the perceived fairness of the wage for a significant fraction of workers.

Figure 1.5: Fairness Evaluation of the Base Wage and High Wage

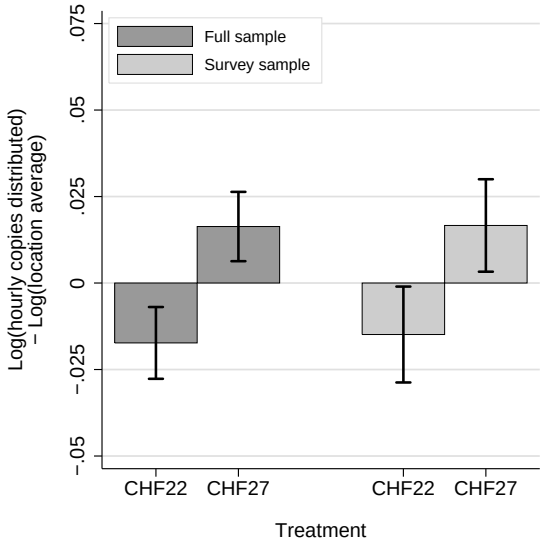


The figure compares anonymous fairness judgments of the base wage (treatment CHF22) and high wage (treatment CHF27).

1.4.2 Impact of Wage Increases on Work Performance

Figure 1.6 presents the overall effect of the wage increases on work performance for both the full sample and for those workers who also participated in the survey. Work performance is measured as the logarithm of the number of hourly copies distributed. Because locations were vacant on some days (see Figure 1.2), the figures in this chapter display work performance as the percentage change in performance compared to the average performance in a certain location. Thus, a zero corresponds to an average work performance, while a positive number indicates greater-than-average performance. We find a rather small overall effect of the wage increases on work performance for both samples. As evident from Figure 1.6, raising the wage by 23 percent increased work performance by about 0.04 log points, or approximately 4 percent.

Figure 1.6: Impact of Wage Increases on Work Performance



The figure presents average work performance per treatment, shown separately for the full and survey sample. Work performance is normalized as the percentage change in the number of hourly copies distributed compared to the average in a certain location.

To examine this effect more formally, we estimate the following equation

$$\log(y_{it}) = \gamma_0 \log(w_{it}) + \psi_{j(it)} + \delta_t + \epsilon_{it}, \tag{1.3}$$

where $\log(y_{it})$ denotes the logarithm of the number of hourly copies distributed by worker i on day t, $\log(w_{it})$ is the logarithm of the hourly wage, which was either CHF 22 or

CHF 27, depending on the treatment. We include location fixed effects ψ_j to control for performance differences between locations, and day fixed effects δ_t to control for differences between days. Finally, ϵ_{it} is the idiosyncratic error term, which we allow to be correlated within workers. We also run a more conservative specification in which we additionally include worker fixed effects, denoted by α_i, to control for differences between workers. The regression equation is then given by

$$\log(y_{it}) = \gamma_0 \log(w_{it}) + \alpha_i + \psi_{j(it)} + \delta_t + \epsilon_{it}. \tag{1.4}$$

We estimate equations (1.3) and (1.4) using OLS, and adjust the standard errors for clustering on workers. The results are displayed in Table 1.4, and the estimated coefficients can be interpreted directly as elasticities. Depending on the sample and specification of the regression model, the point estimate of the treatment effect varies between 0.175 and 0.134. This implies that doubling the wage would have led to a performance increase between 17.5 and 13.4 percent. Our point estimate is small, but it is solidly within the confidence intervals of earlier field studies. Unlike the earlier studies, however, the large number of observations in our study provides enough precision to reject the null hypothesis of no effect.

Table 1.4: Impact of Wage Increases on Work Performance

Dependent variable: Log(hourly copies distributed)	(1)	(2)	(3)	(4)
Log(w_{it})	0.175**	0.134*	0.175*	0.110
	(0.078)	(0.078)	(0.100)	(0.105)
Constant	4.786***	5.139***	4.770***	5.248***
	(0.251)	(0.271)	(0.315)	(0.333)
Individual fixed effects	No	Yes	No	Yes
Location fixed effects	Yes	Yes	Yes	Yes
Day fixed effects	Yes	Yes	Yes	Yes
Sample	Full	Full	Survey	Survey
N	1269	1269	722	722
R^2	0.597	0.716	0.590	0.716
Prob> χ^2, F	0.000	0.000	0.001	0.000

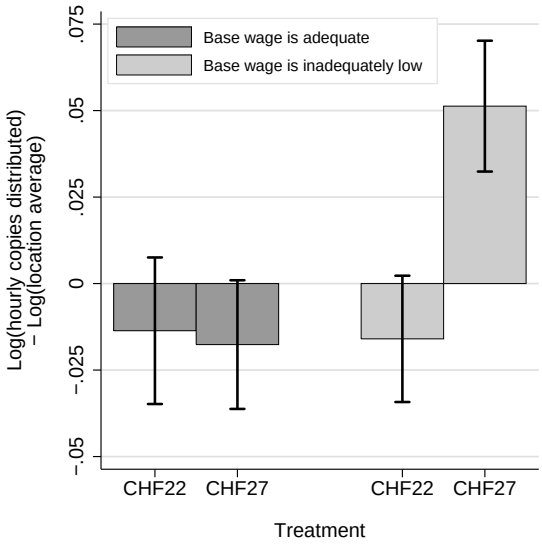
OLS estimates. Standard errors, adjusted for clustering on workers, are in parentheses. The dependent variable is the logarithm of the number of hourly copies distributed. The independent variable $\log(w_{it})$ is the logarithm of the hourly wage. Fixed effects are normalized such that the constant reflects the mean of the omitted category. The sample "Full" involves all workers of the field experiment, while the sample "Survey" includes only the workers who also participated in the follow-up survey. Level of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

1.4.3 Fairness Perceptions and the Response to Wage Increases

Average results may be misleading for the assessment of the role of fairness concerns because we find substantial heterogeneity in fairness perceptions. As already mentioned, about half of the workers considered the base wage to be unfairly low, while the other half perceived it to be fair. We thus examine the impact of the wage increases separately for workers who considered the base wage to be adequate and those who considered it to be inadequately low. As can be seen in Figure 1.7, raising the wage of workers who were already adequately paid had no impact on work performance. Conversely, raising pay of underpaid workers clearly increased work performance. In this case, the standard error bands of the treatment averages do not overlap, indicating a significant difference between treatment CHF22 and CHF27.

These standard errors are, however, calculated under the assumption that each observation is an independent draw. Since we have multiple observations per worker, these standard errors may be too small. To address this problem, as well as to include tighter

Figure 1.7: Fairness Perceptions and the Response to Wage Increases



The figure illustrates average work performance per treatment, shown separately for workers who initially felt adequately paid and those who felt underpaid. Work performance is normalized as the percentage change in the number of hourly copies distributed compared to the average in a certain location.

controls, we estimate the following regression

$$\log(y_{it}) = \gamma_0 \log(w_{it}) + \gamma_1 \Delta_i + \gamma_2 \log(w_{it}) \times \Delta_i + \psi_{j(it)} + \delta_t + \epsilon_{it}, \tag{1.5}$$

where the variable Δ_i is the difference between the wage workers considered to be fair for this work and the base wage. Thus, positive numbers mean that workers considered the base wage to be unfairly low, a value of zero means that the base wage corresponded exactly to the fair wage, and negative numbers imply that workers considered the base wage to be more than fair (though this hardly ever occurred, as Figure 1.4 indicates). We re-estimate this model, but include worker fixed effects. This specification has the advantage that it does not require Δ_i to enter equation (1.5) linearly, but allows for any relationship between individual characteristics, including fairness perceptions, and work performance. The regression equation is then given by

$$\log(y_{it}) = \gamma_0 \log(w_{it}) + \gamma_2 \log(w_{it}) \times \Delta_i + \alpha_i + \psi_{j(it)} + \delta_t + \epsilon_{it}. \tag{1.6}$$

The estimates for equations (1.5) and (1.6) are shown in Table 1.5 and confirm the visual impression from Figure 1.7. As shown in Column (1), the coefficient of $\log(w_{it})$ is not

significantly different from zero. Because of the interaction with Δ_i, this coefficient represents the impact of a wage increase on work performance when Δ_i is zero, i.e., when the base wage was considered to be appropriate. Since we specify both sides of the equation (1.5) in logs, the coefficient of $\log(w_{it})$ can be interpreted directly as an elasticity. Its point estimate is 0.07, and thus, very low. This implies that doubling the wage would have led to 7 percent higher work performance. The 90 percent confidence interval ends at 25 percent, which is still very small. Raising the wage thus had no significant impact on work performance for workers who considered the base wage to be adequate.

In contrast, we find a significant effect from raising pay for workers who considered the base wage to be unfairly low. The coefficient of the interaction between $\log(w_{it})$ and Δ_i is significantly different from zero, implying that underpaid workers responded to the wage increases in a significantly different way than workers who thought that the base wage was fair. The point estimate of this interaction term implies that for every CHF that a worker felt underpaid, the elasticity of work performance with respect to pay rises by 0.093. In other words, for a worker who thought that the hourly fair wage was CHF 27, the elasticity is $0.07 + 5 \cdot 0.093 = 0.54$.

We obtain the same result for the fixed effect specification, as shown in Column (2). There is virtually no response from workers who felt paid adequately in the baseline treatment, but a substantial response from workers who felt underpaid. Thus, heterogeneity in fairness perceptions led to a different response to the wage increases.

Table 1.5: Fairness Perceptions and the Response to Wage Increases

Dependent variable: Log(hourly copies distributed)	(1)	(2)
$\text{Log}(w_{it})$	0.067	0.010
	(0.108)	(0.109)
Δ_i	–0.281***	–
	(0.104)	
$\text{Log}(w_{it}) \times \Delta_i$	0.093***	0.091**
	(0.033)	(0.040)
Constant	5.107***	5.249***
	(0.347)	(0.326)
Individual fixed effects	No	Yes
Location fixed effects	Yes	Yes
Day fixed effects	Yes	Yes
Sample	Survey	Survey
N	722	722
R^2	0.599	0.718
Prob$> \chi^2, F$	0.000	0.000

OLS estimates. Standard errors, adjusted for clustering on workers, are in parentheses. The dependent variable is the logarithm of the number of hourly copies distributed. The independent variable $\log(w_{it})$ is the logarithm of the hourly wage. Fixed effects are normalized such that the constant reflects the mean of the omitted category. Δ_i is the difference between the wage a worker considered to be fair and the base wage (treatment CHF22). The sample is restricted to the workers who participated in the follow-up survey. Level of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

1.4.4 Robustness Checks

We now turn to some robustness checks. First, we test whether this interaction effect is not due to other variables that may happen to be correlated with workers' fairness perceptions. Using data from the follow-up survey, we do not find strong predictors of fairness perceptions. We thus look at ability, as one may assume that high-ability workers are more likely to feel underpaid than low-ability workers. As a result, fairness perceptions could simply reflect worker ability, which in turn could have affected the response to the wage increases.

We interpret worker fixed effects α_i from equation (1.4) as a measure of worker ability. Workers with a high α_i were, on average, more productive than workers with a low α_i. Thus, we divide workers into two groups: workers with an α_i above the median (high-ability), and those with an α_i below the median (low-ability). We re-estimate

equations (1.3) to (1.6), yet include a dummy for high-ability and interact this dummy with $\log(w_{it})$. The results are displayed in Table 1.6 and provide no evidence of such an alternative explanation. Including a dummy for high-ability does not change the conclusions, and the dummy does not interact with workers' response to the wage increases.

Table 1.6: Robustness Check for Ability

Dependent variable: Log(hourly copies distributed)	(1)	(2)	(3)	(4)
$\text{Log}(w_{it})$	0.147	0.074	0.062	0.009
	(0.169)	(0.190)	(0.170)	(0.184)
Δ_i			–0.291***	–
			(0.105)	
$\text{Log}(w_{it}) \times \Delta_i$			0.094***	0.091**
			(0.033)	(0.041)
High-ability (=1)	0.139	–	0.288	–
	(0.739)		(0.738)	
$\text{Log}(w_{it})$ × high-ability (=1)	0.035	0.068	–0.013	0.001
	(0.230)	(0.251)	(0.230)	(0.252)
Constant	4.716***	5.243***	4.987***	5.249***
	(0.537)	(0.330)	(0.540)	(0.323)
Individual fixed effects	No	Yes	No	Yes
Location fixed effects	Yes	Yes	Yes	Yes
Day fixed effects	Yes	Yes	Yes	Yes
Sample	Survey	Survey	Survey	Survey
N	722	722	722	722
R^2	0.652	0.716	0.658	0.718
Prob> χ^2, F	0.000	0.000	0.000	0.000

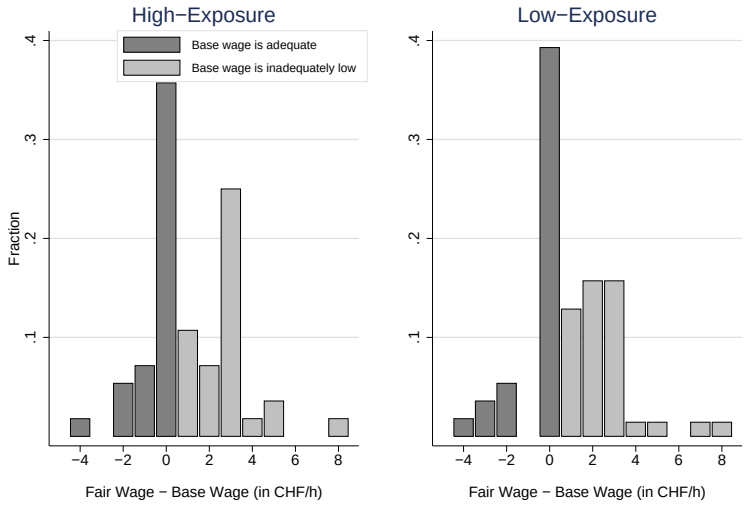
OLS estimates. Standard errors, adjusted for clustering on workers, are in parentheses. The dependent variable is the logarithm of the number of hourly copies distributed. The independent variable $\log(w_{it})$ is the logarithm of the hourly wage. Fixed effects are normalized such that the constant reflects the mean of the omitted category. Δ_i is the difference between the wage a worker considered to be fair and the base wage (treatment CHF22). "High-skilled" is a dummy for worker ability, which takes the value one if a worker's fixed effect is above the median and a zero otherwise. The sample is restricted to the workers who participated in the follow-up survey. Level of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Second, because we measured fairness perceptions ten weeks after the field experiment, we examine whether exposure to the high wage affected the fairness perceptions. Workers with a high exposure to treatment CHF27 may have considered the base wage to be more

unfair, meaning that fairness perceptions could be just a proxy for exposure to the high wage.

Exposure is measured as the fraction of shifts a worker received the high wage. We divide workers into two groups: workers with an exposure above the median (high-exposure), and those with an exposure below the median (low-exposure). Figure 1.8 shows the distribution of fairness perceptions in the two groups. The figure suggests a similar pattern for both groups and a Mann-Whitney rank sum test cannot reject the null that the two distributions are the same ($p = 0.909$).

Figure 1.8: Robustness Check for Exposure to the High Wage



This figure plots the difference between the wage workers considered to be fair for this job and the base wage (treatment CHF22), shown separately for workers who were often and those who were rarely exposed to the high wage (treatment CHF27).

To provide a more rigorous test, we additionally regress individual fairness perceptions on exposure to the high wage using OLS. The point estimate of exposure is 0.19 and thus very low, as shown in Table 1.7. This means that raising the fraction of high-wage shifts from zero to hundred percent increases the wage workers considered to be fair by CHF 0.19. The 90 percent confidence interval ends at CHF 1.3, which is still modest considering that the hourly fair wage varies by as much as CHF 12.

Table 1.7: Robustness Check for Exposure to the High Wage

Dependent variable: Fair wage – base wage (in CHF/h)	
CHF27 exposure	0.190
	(0.684)
Constant	0.992**
	(0.431)
N	119
R^2	0.000
Prob> χ^2, F	0.782

OLS estimates. Standard errors, adjusted for clustering on workers, are in parentheses. The dependent variable Δ_i is the difference between the wage a worker considered to be fair and the base wage (treatment CHF22). The independent variable "CHF27 exposure" is the fraction of shifts a worker received the high wage (treatment CHF27). Fixed effects are normalized such that the constant reflects the mean of the omitted category. The sample is restricted to the workers who participated in the follow-up survey. Level of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

1.4.5 Fairness Preferences and the Response to Wage Increases

Having found evidence that workers' response to higher wages strongly depends on their fairness perceptions, we now seek to analyze the role of fairness concerns in more detail. As mentioned above, second mover decisions in the social dilemma experiment allow us to classify workers as reciprocal and non-reciprocal. According to the reciprocity criterion, 77 out of 118 workers displayed reciprocal fairness.

Thus, we re-estimate equation (1.6) separately for the reciprocal and non-reciprocal workers. Column (1) of Table 1.8 shows the results for the estimates of equation (1.6) restricted to the reciprocal workers, while Column (2) shows the same for the non-reciprocal. The results in Column (1) reveal that the coefficient of $\log(w_{it})$ is not significantly related to work performance. This coefficient represents the impact of a wage increase on work performance of reciprocal workers who considered the base wage as appropriate. However, the point estimate of the interaction between $\log(w_{it})$ and Δ_i is large and positive, implying that for every CHF that a reciprocal worker felt underpaid, the elasticity of work performance with respect to wage rises by 0.16. Put another way, the elasticity is -0.02 + 5·0.16 = 0.78 for reciprocal workers who thought the fair wage was CHF 27.

We find no evidence at all of reaction to the wage increases for the non-reciprocal workers, no matter whether they felt underpaid, as shown in Column (2) of Table 1.8.

Even if we compute a very strict difference-in-difference test, there is clear evidence that the reciprocal workers responded differently to the wage increases than did the non-reciprocal ($p = 0.07$).[15]

Thus, our results demonstrate that when the wage was raised towards the level workers thought fair, only the reciprocal workers responded to the wage increases with higher work performance.

Table 1.8: Response of Reciprocal and Non-reciprocal Workers to the Wage Increases

Dependent variable: Log(hourly copies distributed)	(1)	(2)
Log(w_{it})	–0.017	0.306
	(0.129)	(0.253)
Log(w_{it})×Δ_i	0.156***	–0.094
	(0.047)	(0.097)
Constant	4.792***	5.646***
	(0.389)	(0.753)
Individual fixed effects	Yes	Yes
Location fixed effects	Yes	Yes
Day fixed effects	Yes	Yes
N	466	243
R^2	0.760	0.783
Prob> χ^2, F	0.000	0.000

OLS estimates. Standard errors, adjusted for clustering on workers, are in parentheses. The dependent variable is the logarithm of the number of hourly copies distributed. The independent variable log(w_{it}) is the logarithm of the hourly wage. Fixed effects are normalized such that the constant reflects the mean of the omitted category. Δ_i is the difference between the wage a worker considered to be fair and the base wage (treatment CHF22). Column (1) shows the estimates for the reciprocal, Column (2) shows the same for the non-reciprocal workers. The sample is restricted to the workers who participated in the social dilemma experiment. Level of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

[15]More formally, denote the coefficient estimates for log(w_{it}) and log(w_{it}) × Δ_i for the reciprocal workers by $\gamma^{rec} = (\gamma_0^{rec}, \gamma_2^{rec})'$, and for non-reciprocal workers by $\gamma^{no} = (\gamma_0^{no}, \gamma_2^{no})'$, respectively. Then, $(\gamma^{rec} - \gamma^{no})'(\Sigma^{rec} + \Sigma^{no})^{-1}(\gamma^{rec} - \gamma^{no})$ will be asymptotically $\chi^2(2)$-distributed, where Σ^i denotes the relevant parts of the covariance matrix for group i. This is equivalent to estimating a fully interacted model for the two worker types and testing for significant interaction effects in the response to the wage increases between the reciprocal and non-reciprocal workers.

1.5 Conclusion

This chapter provides evidence on whether higher wages motivate workers to provide more work effort voluntarily. We find that raising the wage caused a small but significant increase in work performance. However, the small overall effect masks substantial heterogeneity in workers' response to wage increases. Our study allows us to trace whether workers' response is related to their fairness perceptions and preferences. We show that the overall effect is purely driven by workers who both initially felt underpaid and who revealed a propensity for reciprocal fairness. The estimated elasticity of the effect is 0.78 for these workers, which means that raising the wage by 10 percent increases work performance by 7.8 percent. By contrast, selfish workers or those who considered the base wage to be appropriate did not respond to pay raises. Their behavior is significantly different from the group of fair-minded workers who perceived the base wage as unfairly low. We consider several possible confounds that would threaten the validity of our estimates, but find no evidence for this. On the whole, our evidence strongly corroborates the fair-wage effort hypothesis put forward in Akerlof and Yellen (1990).

Our results have important implications for wage setting and provide a potential reconciliation for the seemingly incompatible findings in laboratory and field experiments. It is a feature of all earlier field studies that the base wage was comparatively high, meaning that many subjects, as in our study, may have already felt paid fairly before a pay raise. Yet, our findings show that workers are only likely to work more if they initially feel underpaid. This implies that a wage increase only affects work performance if it alleviates perceived unfairness, but not if it merely improves an already fair outcome. When we apply the methodology of earlier studies, and ignore how fairness perceptions and preferences affect workers' response to wage increases, we also find a modest effect. Thanks to a sample much larger than in earlier studies, however, we have enough power to reject the hypothesis of no effect.

Our finding that higher wages only affect work performance when workers feel treated unfairly is consistent with Mas (2006), who finds that police officers' performance is very sensitive to disappointing arbitration outcomes in wage bargaining. In contrast, Mas (2006) finds little evidence that performance is sensitive to the size of an unexpectedly good outcome. It is tentative to conclude that disappointing outcomes were below what the police officers perceived to be fair. Given that our results share these two qualitative features, this channel appears particularly plausible.

From a methodological point of view, our results highlight the importance of complementing field experiments with additional outside information (for example, through surveys or lab experiments) that allow a deeper examination of the psychological mechanisms that drive the response to an experimental intervention. We not only show that fairness perceptions modulate workers' response to a wage increase, but that preferences do so as well. As in lab experiments, we find that not all individuals respond to a change in fairness. Our additional "lab" experiment reveals that only fair-minded individuals respond to changes in fairness, while selfish individuals do not. Hence, the composition of the workforce not only in terms of fairness perceptions but also with respect to preferences determines the effectiveness of a wage increase. Because of many potential confounds, an exact measurement of preferences is seldom possible in a field study. Therefore, our results also underline the usefulness of lab experiments for understanding field behavior.

It is difficult to find plausible alternative interpretations of our findings. In particular, one interpretation that has received attention in previous work does not apply here. Al-Ubaydli et al. (2008) find that treating workers more generously increases work performance. They interpret their results in terms of a repeated-game context, as there is at least some scope for reputation incentives in their setting.[16] These concerns would have contradicted the results we found in our setting. If workers feared being dismissed for insufficient work effort, we should expect an interaction effect of the opposite sign. Workers who felt well-paid would have had more at stake if they were fired. They should have been particularly sensitive to the employer's demands and should have reacted more to the wage increases. But we find that they responded less, which is inconsistent with this interpretation.

This study also points to new questions for future research. Our results suggest that firms should increase workers' wages if they feel underpaid rather if they feel adequately paid. This is because underpaid workers exhibit a greater elasticity of work performance with respect to wage. Little is known, however, about how workers respond to changes in coworkers' wages. In addition, more research is needed to understand how managers can influence workers' fairness perceptions and how these perceptions adjust to labor market conditions.

[16]They do not explicitly test this channel in their field experiment, for example, they do not compare the outcomes to a subset of subjects certain to leave this job. It is therefore difficult to conclusively attribute the effects to reputation incentives. See List (2006) for a field study that explicitly tests the role of reputation incentives in a sports card marketplace.

Chapter 2

Social Comparison in the Workplace

Chapter Summary

We conducted a field experiment to test whether workers respond to wage cuts and whether their response depends on coworkers' wages. Workers were organized in teams of two and paid a flat wage. Either one or both workers in a team suffered a wage cut, while workers in the control treatment continued to earn the initial wage. We show in a difference-in-differences analysis that cutting both workers' wage reduced work performance significantly. However, cutting only one worker's wage resulted in a decrease in performance that was twice as large. In contrast, the spared worker's performance remained unaffected. These findings corroborate the fair wage-effort hypothesis, which can explain involuntary unemployment and wage compression.

2.1 Introduction

Standard economic models of labor markets assume that workers respond exclusively to economic incentives and only care about the absolute level of income. These models do not take into account that fairness motives and social comparison may determine worker behavior. The relevance of these motivational forces, however, was long emphasized in social psychology (Festinger, 1954; Homans, 1961; Adams, 1963) and sociology (Davis, 1959; Runciman, 1966; Pollis, 1968). Textbooks on personnel management also regard the need for fair and equitable treatment of workers as obvious. Kochan and Barocci (1985) quote approvingly from a War Labor Board project (by William H. Davis):

> There is no single factor in the whole field of labor relations that does more to break down morale, create individual dissatisfaction, encourage absenteeism, increase labor turnover and hamper production than obviously unjust inequalities in the wage rates paid to different individuals in the same labor group within the same plant.

These ideas have since been partially integrated into the economic theory of labor markets. A well-known example is the fair wage-effort model by Akerlof and Yellen (1990) which states that workers' fairness concerns constrain firms' wage setting. The model draws on two key assumptions: first, workers withhold effort when they perceive that they are paid less than the fair wage, and second, the fair wage depends on the wages paid to the coworkers in the same firm. Under these assumptions, the firm's wage setting may lead to involuntary unemployment and wage compression.

In this chapter we report evidence from a randomized field experiment exploring whether workers respond to wage cuts and the extent to which wages paid to coworkers influence their responses. We conducted the field experiment in collaboration with a firm that sells a card permitting customers to attend parties at selected bars and nightclubs. The firm hired workers for two weeks to sell a promotional card at a price of €5 or in exchange for customer data. Workers had to work in teams of two.[1] In the pre-intervention week, all workers earned a flat base wage. In the post-intervention week, teams were randomly assigned to one of three treatments: in the control treatment, workers continued to earn the base wage; in the second treatment, both workers in a team

[1] Team members worked independently of each other, i.e., we observe the amount of sold cards for each worker individually.

suffered a wage cut; in the third treatment, only one randomly chosen worker in a team suffered a wage cut, while the other worker continued to earn the base wage. To rule out reputation incentives, workers' employment was limited to two weeks without the prospect of future employment.

We show that workers' performance largely depends on the wages paid to coworkers. When only one worker in a team suffered a pay cut while the coworker was spared, performance of the worker with the lower wage declined sharply by 34 percent. In contrast, when a worker suffered the wage cut along with the coworker, work performance only decreased by 15 percent. This difference in responses is highly significant. When workers were spared from the wage cut while their coworker was not, the spared workers' performance was not affected.

To our knowledge, the present study is the first field experiment to examine the causal effects of wage differentials on worker performance. Empirical evidence on the relevance of social comparison in the workplace has only recently emerged, and the results are mixed. Previous findings stem primarily from laboratory experiments. Aside from the debate whether results from laboratory settings can be generalized to the field (Falk and Heckman, 2009), laboratory experiments entail additional issues when studying social comparison effects.

First, natural and salient references for comparison are poorly induced because subjects in laboratory multi-worker firms typically do not interact with each other on a personal level. The fact that they often do not compare themselves with their coworkers is thus not particularly surprising in this case. For example, in Gächter et al. (2008), subjects did not interact, other than observing anonymous coworkers' wages and effort levels before choosing own effort. As a result, subjects did not respond to coworkers' wages and effort levels. In our field experiment, we created a natural and salient person for comparison by forming teams of two workers who were employed together on two consecutive weekends.

Second, implementing unequal wages in the laboratory is problematic when subjects play the role of the firm and workers are essentially identical. Wage differentials are then rarely observed because there is no basis for wage discrimination. To address this problem, Charness and Kuhn (2007) introduced productivity differentials among workers. Although unequal wages were common, workers did not respond to wages paid to coworkers. Unequal wages might not be perceived as unfair when workers know that they differ in productivity. Instead of implementing productivity differentials, Thöni and Gächter

(2009) applied the strategy method to elicit responses to wage differentials. Their results lend some support for social comparison, but are inconclusive on the whole. In our study, differences between workers did not justify the wage differentials we exogenously introduced. In fact, any justification of wage differentials mitigates social comparison effects. For example, Hennig-Schmidt et al. (forthcoming) justified unequal wages by informing one group of workers that less money was available for them than for the other group. This explanation might have made workers consider wage differences acceptable, and therefore avoids a social comparison effect. The fair wage-effort model, however, posits that only wage changes that also affect wage fairness induce effort variations. We therefore purposely left wage differentials unjustified.

Non-experimental field studies are not better suited for identifying social comparison effects because factors unobservable to the researcher may determine both wage differentials and effort choices (Shearer, 2003).[2] We address this endogeneity problem by randomizing wage differentials.

Much of our previous understanding about the harmful effects of wage cuts on work motivation relies on laboratory experiments and manager interviews. Fehr and Falk (1999), for example, conducted a laboratory experiment to investigate downward wage rigidity in competitive labor markets. They find that firms fear low and unprofitable effort levels as a consequence of low wages and are therefore reluctant to accept low wage offers from previously unemployed workers. As a result, wages settle above the competitive level.

Bewley (1999) provides an example for an interview study on wage stickiness. He reports that managers are reluctant to cut pay during a recession because they are afraid of the negative effects on work morale. Although interview studies are suggestive, the findings only reflect managers' opinions and not worker behavior.[3]

In a non-experimental study, Lee and Rupp (2007) examine the effects of wage cuts on flight delays in the airline industry. They find that wage cuts trigger hostile responses only when the cut is perceived to be unfair. As for wage differentials, non-experimental field studies on wage cuts do not solve the endogeneity problem. In addition, non-experimental field studies examine worker behavior in ongoing employment relations, making it difficult

[2]Torgler et al. (2008) investigate the relevance of social comparison among basketball and soccer players. They find that either situation – earning more *or* earning less than teammates – reduces performance.

[3]For example, managers may be reluctant to cut wages because they would have to deal with offended workers who might contest the situation; this would not necessarily imply, however, that the workers would work less.

to disentangle fairness motives from reputation incentives (Howitt, 2002).[4]

To date, only Kube et al. (2010*a*) provide clean evidence on the negative effects of wage cuts on worker performance. In their field experiment, workers reduced their performance significantly when the wage was lower than the promised wage.

2.2 Experiment

2.2.1 Economic Environment

We conducted the field experiment in collaboration with a German firm that operates a nightlife online portal and sells a card that permits attendance to parties in selected bars and nightclubs. In 2008, the firm initiated a promotion to increase its brand awareness. For this promotion, it hired workers to sell a promotional card in public places and nightclubs.

2.2.2 Recruitment

Workers were hired over a job market database that listed workers with experience in promotion jobs. They were unaware that they were participating in an experiment. To ensure this, we excluded applicants who knew someone from the firm's permanent staff. Hires received a guarantee that they could work on two consecutive weekends and it was made clear that there was no prospect of further employment at the firm.[5]

Upon arriving for the training session, workers were randomly allocated to teams of two and randomly assigned to be either "worker 1" or "worker 2" in a team. Both workers, however, had completely identical tasks and responsibilities. By forming teams of two workers who worked together on two consecutive weekends, we created a natural and salient reference for comparison, namely the coworker in a team.

Workers were then informed about the objective of the promotion, its structure and procedures, as well as with the equipment and clothing. Furthermore, they were trained how to approach potential customers.

[4]For example, when firms cut wages, workers may simply punish the firm with lower effort as part of an equilibrium trigger strategy.

[5]This eliminated reputation incentives.

2.2.3 Task

Workers' task was to sell the promotional card at a price of €5 or in exchange for a customer's personal information[6]. Teams were assigned a fixed point-of-sale which was either a public place or a nightclub. Working hours were Friday and Saturday from 5pm to 8pm for public places, respectively 11pm to 2am for nightclubs.

During these three-hour shifts, workers were mostly on their own and thus had full discretion over the amount of exerted effort. In addition, points-of-sale provided an attractive opportunity to shirk because workers could unobtrusively converse privately, and they could always claim that nobody wanted to buy the card.

Each team was managed by a team leader, who met the workers before and after work shifts. Team leaders supplied workers with promotional cards, assessed the points-of-sale (for example, number of club visitors), and looked after the workers once or twice per shift. After the shifts, they collected the revenues, customer information, and the remaining cards.[7]

2.2.4 Treatments

We implemented a differences-in-differences setup with a pre- and a post-intervention week and four treatment groups (HH, LL, HL1, and HL2). The pre-intervention week permits the measurement of workers' baseline performance, thus controlling for worker heterogeneity. This baseline is important because ability and therefore performance may vary strongly across workers.

In the pre-intervention week, all workers earned an hourly base wage of €12. For the post-intervention week, we randomly assigned teams to one of three treatments. In the control treatment, "HH", both worker 1 and worker 2 continued to earn the hourly base wage of €12.[8] In the second treatment, "LL", both worker 1 and worker 2 suffered a wage cut down to €9 per hour. In the third treatment, "HL", only worker 2 suffered a

[6]Customer information was recorded in a database, and invitations to join the online platform were sent to the customers. False information could be identified and attributed to the worker who had acquired it. Workers did not know, however, that the correctness of customer information was verified.

[7]Team leaders were permanent employees of the firm and each of them was responsible for two to three teams. They received a comprehensive set of instructions about their communication with the workers and the handling of potentially problematic situations. In particular, they were instructed to treat all workers in the same manner and were prohibited to motivate or rebuke individual workers.

[8]Team leaders used the phrase "You continue to earn €12 per hour. This was the manager's decision." and analogous phrases in the other treatments.

wage cut down to €9 per hour, while worker 1 still earned the base wage of €12 per hour (see Table 2.1 for a summary of the treatments). Thus, treatment HL consists of two groups: group "HL1" is composed of workers 1 in treatment HL, while "HL2" is made up of workers 2.

Table 2.1: Hourly wages (in €)

Treatment	HH		LL		HL	
Worker	1	2	1	2	1	2
Pre-intervention	12	12	12	12	12	12
Post-intervention	12	12	9	9	12	9

Team leaders did not give a reason for the wage cut to prevent altering the wage that workers consider fair. For example, workers' notion of a fair wage could have changed if team leaders had told that the firm faced the risk of bankruptcy. In addition, devising a cover story would have deceived the workers, which would have been a departure from standard convention among experimental economists.

2.2.5 Design Specifics

Three important aspects of this field experiment need to be stressed. First, we implemented a wage cut so that workers earned, on average, at least the promised wage. Thus, we initially raised all workers' hourly wage from €10 to €12.[9] This avoids ethical concerns associated with experimental pay cuts. In addition, the wage increase helps prevent an attrition bias. Drop-outs after a wage cut would be uninformative because they could be interpreted either as a hostile response or the choice of an outside option because the wage had fallen below a worker's reservation wage. This initial wage increase, however, is associated with the potential cost of mitigating the treatment effects.

Second, we adapted the organizational structure of the promotion to maximize the number of subjects. We ran the promotion twice, hiring different workforces each time.[10] In addition, we ran the promotions in two cities and at two different types of points-of-sale in each of the promotion drives. This made some balancing constraints on the treatment

[9]Upon being hired, workers were promised an hourly wage of €10. Then, at the beginning of the first shift, team leaders informed the workers about the wage increase. If workers asked for a reason, they were told that the manager had made the decision.

[10]Workers from the first promotional drive never had contact with workers from the second drive.

assignment desirable.[11] We assigned treatments evenly to the two points in time, cities, and types of points-of-sale. These constraints minimize time, city, and location type-specific differences across treatments. To maximize statistical power, each of the four treatment groups comprised the same number of workers (i.e., treatment HL comprised as many workers as treatments HH and LL together). We also ran treatment HL at each point-of-sale: the first time at half of the points-of-sale, and the second time at the other half (i.e., each point-of-sale was assigned to treatment HL once and to either treatment HH or LL the other time). Furthermore, we stratified treatment assignment by gender, and also formed same gender teams to avoid confounds.[12]

Third, we allocated workers who knew each other to the same treatment, thus preventing communication among workers from different treatments in order to avoid treatment contamination. We separated friends into different teams, however, to preclude friendship arrangements within teams. In addition, allocation of teams in time and space ensured that teams from different treatments could not possibly meet.

2.3 Behavioral Predictions

Using a simple framework, we analyze how workers respond to wage cuts and how their response depends on the wages paid to their coworkers. Consider a firm that employs two identical workers, worker 1 and worker 2, for a one-time job and pays them a flat wage. Each worker in return generates revenue for the firm by exerting costly effort. The firm's payoff per worker is revenue generated by the worker minus her wage. The worker's payoff is the wage minus her effort cost.

Assume first that workers exclusively maximize their own material interest. The prediction is then straightforward: since workers receive a guaranteed wage that is not contingent on their performance, their effort will not respond to a change in the flat wage as long as the wage remains above their reservation wage.[13]

[11] Assignment to worker 1 and worker 2 was randomized unconditionally.

[12] For example, HL2 workers in mixed-gender teams might reduce their performance because they felt they were victims of sexual discrimination.

[13] Alternatively, a high wage could be regarded as a disciplining device for selfish workers (Shapiro and Stiglitz, 1984): workers provide high effort to avoid being fired and losing the high wage. After a wage cut, workers decrease their effort because they have less to lose. This model predicts a general drop in effort towards the end of the employment and, after a wage cut, the same decrease in effort regardless of the coworker's wage. In our setup, however, this theory is implausible because there was no threat of firing.

Now assume that in addition to their own material interest, workers care about fairness, and their fairness perceptions are based on either the actions or outcomes of others.[14] For example, workers could view a wage cut as a hostile act by the firm and as a consequence reduce their effort. If only one worker's wage is cut, he or she could consider this act even more hostile and further amplify the effort reduction. Alternatively, inequalities in outcome might influence worker behavior. Outcome-oriented fairness models have the advantage of being tractable. We therefore use the model of inequity aversion by Fehr and Schmidt (1999) to generate our hypotheses,[15] and to derive the formal predictions in the Appendix.

Suppose that workers dislike inequity when comparing their own payoff, that of their coworkers, and the profit the firm earns. Not only effort cost, but payoff comparison as well, determines a worker's effort. Higher effort decreases own payoff and increases the firm's payoff, but leaves the coworker's payoff unaffected.

With regard to the firm, wage payment by definition creates inequity to the workers' advantage. The firm-worker payoff difference is proportional to the wage. Thus, the higher the wage, the more effort workers must provide in order to eliminate this inequity. In treatments HH and LL, both team members are paid the same wage. To avoid inequity between them, they will both provide the same effort. To prevent inequity with respect to the firm, workers' effort will be lower in treatment LL than in HH.

In treatment HL, the firm pays worker 2 a lower wage than worker 1. In this case, payoff comparisons both with the firm and with the coworker affect effort choices. Consider worker 1 first: provided that both workers exert the same level of effort as in HH, worker 1 receives the same payoff as the firm but a *higher* payoff than worker 2. Consequently, worker 1 could increase her effort in order to reduce advantageous inequity with respect to her coworker. This, however, would increase not only effort cost but also overall inequity: while an increase in effort decreases the inequality between workers as worker 1's payoff is reduced, it creates a larger inequality between her and the firm through both a reduction in her own payoff and an increase of the firm's payoff. Worker 1 will therefore not exert more effort; as a result, HL1 effort will be the same as in treatment HH.

Consider now worker 2: provided that both workers exert the same level of effort as in

[14] Action-oriented fairness models include Rabin (1993), Dufwenberg and Kirchsteiger (2004), Falk and Fischbacher (2006); outcome-oriented fairness models include Fehr and Schmidt (1999) and Bolton and Ockenfels (2000); Levine (1998) presents a type-based fairness model.

[15] The purpose of our experiment is to provide causal evidence on the effect of social comparison on work performance and not to discriminate between closely related fairness models.

HH, worker 2 receives a *lower* payoff than the firm and worker 1. Hence, reducing effort decreases not only effort cost but also inequity with respect to both the firm and the coworker. In order to equalize payoffs with respect to the firm, worker 2 would provide the same low effort level as in LL. Yet, at this effort level, worker 2 still gets a smaller payoff than worker 1.[16] If worker 2 further decreases her effort, she not only saves effort cost but also reduces disadvantageous inequity with respect to her coworker; this comes at the cost of a disparity between her and the firm to her advantage. This cost, however, is small because envy looms larger than compassion. Worker 2 therefore provides less effort than when both workers earn the low wage; consequently, HL2 effort is lower than in treatment LL.

In summary, the model generates the following hypotheses about the change in effort from pre- to post-intervention week:

if workers are sufficiently inequity averse, then

(H1) workers in treatment **LL** decrease their effort after the wage cut.

(H2) workers in treatment group **HL2** decrease their effort after the wage cut more than those in treatment LL.

(H3) workers in treatment group **HL1** do not change their effort after the wage cut.

2.4 Results

2.4.1 Descriptive Statistics

Our sample consists of 96 workers in 48 teams. Table 2.2 shows that workers were predominantly women (77 percent) and, on average, in their early twenties (mean age: 20.7 years). All but one was of German nationality; 29 workers, however, had a second nationality (mostly Eastern European). Of the 96 workers, three workers got sick before any wage cut was announced and missed out on the entire post-intervention week.[17] However, no worker dropped out because of the wage cut.

In total, workers sold 8750 promotional cards; mean sales were 22.8 cards per three-hour shift and worker. Only 187 customers (2.1 percent) chose to pay €5 for the card,

[16] Recall that a reduction in effort decreases inequity with respect to the firm more effectively than inequity with respect to the coworker because own effort affects the firm's payoff but it does not affect coworker's payoff.

[17] These workers were replaced by spare workers who were treated in exactly the same way as the replaced workers would have been treated. We exclude spare workers from the analysis, however.

while the remaining sales were generated by collecting customer information. Of the 8563 sets of customer information, only 191 (2.2 percent) were false.

Table 2.2: Summary Statistics

Treatment Group	HH	LL	HL1	HL2	Total
# Workers	24	24	24	24	96
# Female	18	18	19	19	74
Age (mean)	20.5	21.2	20.2	21.1	20.7
# Reported sick	1	1	0	1	3
Sales per shift (mean)					
Pre-intervention	20.8	22.4	24.3	22.0	22.4
Post-intervention	22.8	21.4	26.5	18.4	22.3

2.4.2 Control Variables

We balanced the treatments over variables known in advance (points in time, city, point-of-sale, and gender). Two particular factors, however, were impossible to anticipate, namely how many customers the workers would meet at the points-of-sale (demand), and heterogeneity in worker characteristics. Both factors influence worker performance and can cause spurious correlations in the data if they are not accounted for.

Based on their visits during work shifts, team leaders assessed demand on a 5-point scale (-2 = low, 2 = high). We use this assessment to test for systematic variation in demand across treatments. A Kruskal-Wallis test cannot reject the null hypothesis that demand was equally distributed across treatments ($p = 0.23$).[18] Nevertheless, there is considerable variation in demand (standard deviation: 1.19). In order to estimate treatment effects more precisely, we also report a specification where we include demand as an additional control in the regression analysis.

To control for worker heterogeneity, we implemented a difference-in-differences design which allows us to include individual fixed effects in the regression analysis.[19] Many field studies report substantial heterogeneity in worker ability and point out the importance of controlling for this heterogeneity whenever possible (Shearer, 2004; Fehr and Götte, 2007; Mas and Moretti, 2009).

[18] All p-values in the analysis are two-sided.

[19] As workers were always assigned to the same point-of-sale, individual fixed effects also capture location-specific differences.

2.4.3 Impact of Wage Cuts on Work Performance

Workers could exert effort in two dimensions of performance: quantity (cards sold) and quality (correctness of customer information). Our measure of effort is quality-adjusted performance, defined as the total number of cards sold less sales due to customer data that were verified as incorrect.[20] With random treatment assignment, we can estimate the average causal effect of an intervention by comparing pre- and post-intervention differences in performance across treatments. We first conservatively analyze the impact of the treatments on performance by applying non-parametric tests. For convenience, we normalize quantities as percentages of the pre-intervention performance average $\overline{y}_{pre}$, resulting in the following hypotheses:

(H1) Workers in treatment LL reduce performance compared to those in the control treatment:

$$\frac{y^{LL}_{post} - y^{LL}_{pre}}{\overline{y}_{pre}} < \frac{y^{HH}_{post} - y^{HH}_{pre}}{\overline{y}_{pre}}$$

(H2) Workers in treatment group HL2 reduce performance more than those in treatment LL:

$$\frac{y^{HL2}_{post} - y^{HL2}_{pre}}{\overline{y}_{pre}} < \frac{y^{LL}_{post} - y^{LL}_{pre}}{\overline{y}_{pre}}$$

(H3) Workers in treatment group HL1 provide the same performance as those in the control treatment:

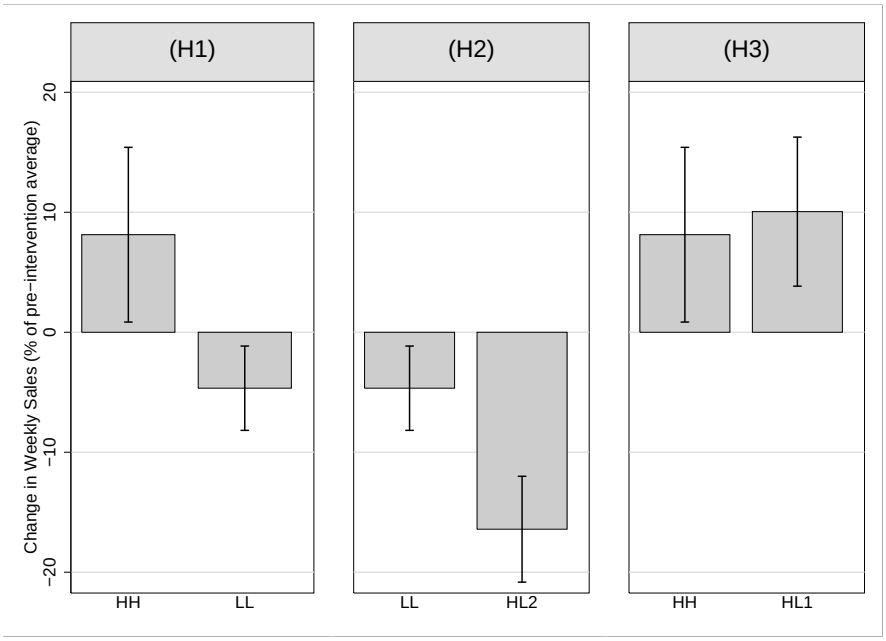
$$\frac{y^{HL1}_{post} - y^{HL1}_{pre}}{\overline{y}_{pre}} = \frac{y^{HH}_{post} - y^{HH}_{pre}}{\overline{y}_{pre}}$$

Figure 2.1 shows the percentage change in performance from pre- to post-intervention week by treatment. Performance in the control treatment HH increased non-significantly

[20] The empirical results do not change if we include incorrect customer data in the analysis.

by 8 percent relative to the pre-intervention average ($p = 0.58$, Wilcoxon Signed Rank), which points to learning effects. Conforming to our hypotheses, we test for differences between this baseline change and changes in the other treatments. Compared to the control treatment, performance in treatment LL decreased non-significantly by 13 percentage points ($p = 0.37$, Mann-Whitney U), lending weak support for hypothesis (H1). By contrast, a wage cut for only one worker had a dramatic impact on performance: workers in the HL2 group significantly decreased their performance by 24 percentage points compared to the control treatment ($p < 0.01$, Mann-Whitney U). This reduction in performance is also stronger than the decrease in the LL group ($p < 0.05$, Mann-Whitney U), providing strong first evidence for hypothesis (H2): workers reacted more drastically to wage cuts when the coworker was spared. Moreover, if we compare the paired observations within HL teams, we see that HL2 workers also reduced their performance compared to their coworkers ($p < 0.01$, Wilcoxon Signed Rank). Finally, the spared coworkers hardly responded to the wage cut: HL1 workers increased their performance by 2 percentage points compared to the control group ($p = 0.37$, Mann-Whitney U), which is consistent with hypothesis (H3).

Figure 2.1: Impact of Wage Cuts on Work Performance



Panel (H1) compares the performance change in treatment group HH (control) and LL (general wage cut), Panel (H2) compares treatment group LL (general wage cut) and HL2 (unilateral wage cut), while Panel (H3) compares treatment group HH (control) and HL1 (spared workers).

Our non-parametric analysis does not control for differences in demand and worker characteristics. To address this issue, we estimate a difference-in-differences regression model that uses the balanced panel data structure with each worker as a panel unit and each team as an independent observation:[21]

$$\log(y_{ikt}) = \alpha + \nu_i + \theta_t + \delta D_{kt} + \beta_1 I_{kt}^{LL} + \beta_2 I_{kt}^{HL1} + \beta_3 I_{kt}^{HL2} + \epsilon_{ikt} \tag{2.1}$$

where $\log(y_{ikt})$ denotes the logarithm of the performance per shift of worker i in team k and week t; the constant α captures the average pre-intervention performance, ν_i represents individual fixed effects, θ_t captures the baseline trend from pre- to post-intervention week, and D_{kt} controls for differences in demand; I_{kt}^g are intervention dummies for whether the respective intervention has affected treatment group g in week t (the omitted category is the control group HH); finally, ϵ_{ikt} is the idiosyncratic error term, which is clustered over teams. Recall that individual fixed effects not only capture time invariant differences across workers but also location-specific factors because workers were always assigned to the same point-of-sale.[22]

Table 2.3 shows the estimates for the treatment effects. Column (1) presents the results for equation (2.1) omitting the control variable for demand. The "Post-intervention" dummy represents the percentage change in baseline performance from pre- to post-intervention week. The coefficient of this dummy shows that performance in treatment HH increased non-significantly by 6 percent ($p = 0.47$, t-Test). The intervention dummies, i.e. the three interactions "$g\times$ Post-intervention", describe how the change in performance differed with respect to the control group. In treatment LL, the change in performance was 11 percentage points lower than in treatment HH; this negative response is twice as large in magnitude as the baseline change, reaffirming hypothesis (H1). However, data are too noisy to reject the null hypothesis of no difference between treatment HH and LL ($p = 0.25$, t-Test). Compared to treatment HH, however, workers in the HL2 group reduced their performance by 31 percentage points ($p < 0.01$, t-Test). They responded three times more strongly to the wage cut than workers in treatment LL ($p = 0.01$, Wald-Test), which strongly corroborates hypothesis (H2). In line with hypothesis (H3), workers in group HL1 did not significantly increase their performance compared to the control group ($p = 0.72$, t-Test).

[21] For an exposition of difference-in-differences, see citetbertrand04.

[22] As expected, the fixed effects parameters are highly significant ($p < 0.001$, F-test).

In order to reduce residual variance, we include demand as a control variable in Column (2).[23] The coefficient of demand is highly significant ($p < 0.01$, t-Test) and has the expected sign: the more potential customers, the likelier workers could sell a card. The inclusion of the demand variable does not qualitatively change the results. It allows us, however, to estimate the treatment coefficients more precisely. As a consequence, the influence of the wage cut affecting both workers is now significant at the 10 percent level. The point estimates for the two wage cut interventions imply a performance reduction of 15 percentage points for the LL group ($p = 0.07$, t-Test) and 34 percentage points for the HL2 group ($p < 0.01$, t-Test). The difference between the LL and the HL2 group is again highly significant ($p = 0.01$, Wald-Test). The coefficient for the HL1 group remains small and insignificant ($p = 0.87$, t-Test).

Table 2.3: Impact of Wage Cuts on Work Performance

Dependent variable: Log(sales)	(1)	(2)
Post-intervention	0.058	0.090
	(0.080)	(0.074)
LL × Post-intervention	−0.106	−0.145*
	(0.090))	(0.079)
HL1× Post-intervention	0.034	0.015
	(0.095)	(0.089)
HL2× Post-intervention	−0.306***	−0.342***
	(0.103)	(0.101)
Demand		0.117***
		(0.034)
Constant	3.057***	3.048***
	(0.015)	(0.015)
Individual Fixed Effects	Yes	Yes
N	189	179
Adj. R^2	0.202	0.312

OLS estimates. Standard errors, adjusted for clustering on teams, are in parentheses. The dependent variable is the logarithm of the total number of cards sold less sales due to customer data that were verified as incorrect. Level of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Taken together, the results in Table 2.3 weakly support hypothesis (H1), but they strongly corroborate hypothesis (H2). The results demonstrate that workers are not

[23]When demand is included in the regression, the number of observations decreases by 10 because the demand measure is missing for some shifts.

only concerned about their own wage. Rather, workers compare themselves with their coworkers and respond more negatively to wage cuts if they end up earning less than their coworkers. Finally, consistent with hypothesis (H3), preferentially treated workers do not increase their effort, pointing out the boundaries of social comparison effects.

2.5 Conclusion

This chapter reports evidence from a randomized field experiment investigating fairness motives and social comparison in a real-life employment situation. When both workers in a team suffered a wage cut of 25 percent, performance declined by 15 percent. We may even underestimate this effect because workers initially received a wage increase. This wage increase partially outweighed the wage cut, thus, workers may have perceived the wage cut as less unfair.

This result provides causal evidence on why firms often refuse to cut wages even though excess labor supply exists and labor markets have not yet cleared. The question about downward wage stickiness has played a key role in a long-lasting macroeconomic debate starting with Keynes in the 1930s. Bewley (1999) was able to show that managers regard fairness as the most important reason for downward wage rigidity. In Bewley's interviews, managers revealed that they were afraid of workers' resentment in response to wage cuts. To date, behavioral evidence for the fairness explanation is still scarce because exogenous wage cuts are rarely observed.[24] We fill this gap by implementing randomized wage cuts in a real-life job of limited duration to exclude explanations other than fairness.

Our main result demonstrates the key role of social comparison in the workplace. When only one worker in a team suffered a wage cut of 25 percent, the affected worker's performance declined, on average, by 34 percent. This effect is much stronger than the effect of the pay cut for both workers in a team, even though the wage was reduced by the same amount. This result provides clear evidence for the conjecture that workers respond to wages paid to their coworkers. In contrast, spared workers did not respond to the wage cut their coworkers suffered. This asymmetric effect speaks to the results of a field experiment by Cohn et al. (2009). They show that workers who feel overpaid do not respond to wage increases, while workers who feel underpaid respond with a performance increase.

This study sheds light on why firms usually avoid paying some workers less than

[24]The only exception known to us is Kube et al. (2010*b*).

others on the same job. In the 1980s, for example, airline companies such as American Airlines, Delta, or Northwest introduced two-tier wage systems where new hires were paid less than incumbent workers. These wage policies, however, were phased out in the 1990s due to the resentment of the workers as well as the high turnover these lower wages generated (Card, 1997). In a New York Times article, Salpukas (1987) noted that two-tier wage systems have "produced a resentful class of workers who in some cases are taking their hostility out on customers". Social comparison may also affect strategic decisions of companies, such as mergers and acquisitions. For instance, the acquisition of Piedmont Aviation by U.S. Airways entailed unexpectedly high acquisition cost due to wage increases at Piedmont to U.S. Airways' more generous salary levels (Kole and Lehn, 2000). Such costly measures are necessary to ensure internal pay equity. When General Electric (GE) acquired NBC in 1986, for example, engineers at GE were angered by the fact that their colleagues at NBC earned higher salaries (Camerer and Malmendier, 2005). Our study provides causal evidence that wage disparities within firms also greatly damage productivity. This provides a plausible reason why firms frequently prefer compensation practices that maintain firm-internal equity such as wage compression (Akerlof and Yellen, 1990) and wage secrecy (Lawler, 1990).

Together, these two findings correspond to the behavioral relation between wages and effort levels described in the phenomenological model by Akerlof and Yellen (1990).[25] It is important to note that other efficiency wage models, such as the well-known shirking model of Shapiro and Stiglitz (1984), cannot explain these results. In particular, this alternative model does not predict that responses to wage cuts depend on coworker wages.

The present study focuses on one specific determinant of workers' fair wage, namely coworkers' wages. Although this may be the most important determinant, there may be others. For example, past wages may influence what workers think they are entitled to (Kahneman et al., 1986). Thus, fairness considerations may also have severe implications for the optimal wage policy over time.

Another important aspect is the communication of wage policies. For example, workers may be willing to accept wage cuts when they feel that they are justified (Greenberg, 1990). Therefore, managers may prevent adverse consequences following from pay cuts if they can thoroughly and sensitively explain the reason for the wage reduction, for example in order to avoid bankruptcy.

[25] As shown in the appendix, this behavioral pattern can be derived from recently developed models of social preferences.

Chapter 3

Cooperation, Collusion and Social Interaction

Chapter Summary

We conducted a controlled field experiment to analyze how social interaction affects work performance under team and relative incentives. Under team incentives, when individual effort imposed a positive externality on the team member's income, social interaction had no impact on work performance. By contrast, under relative incentives, when effort imposed a negative externality, social interaction caused a substantial drop in work performance. Moreover, in teams with homogeneous skills, social interaction also reduced within-team performance differences. Both findings provide evidence that social interaction evoked collusion in the presence of negative externalities. However, collusion grew increasingly difficult the more team members differed in skills. This result points out the limits of collusion.

3.1 Introduction

In many jobs, workers impose positive or negative effort externalities on their coworkers.[1] If workers fail to internalize the externalities, inefficiencies arise for them. Workers can prevent these inefficiencies by making implicit effort agreements. These agreements prescribe the effort level that maximizes mutual rather than private surplus. To be effective as a motivational device, however, agreements need to be enforced by punishing defectors. Social interaction provides workers with the possibility of observing and deterring agreement violations. A fundamental question is thus whether social interaction facilitates cooperation in the presence of positive externalities, and collusion in the presence of effort externalities respectively.[2]

We conducted a field experiment to explore the impact of social interaction on work performance when either positive or negative effort externalities are in place. Workers were recruited to catalogue books in a university library. They were asked to come to work in groups of four and randomly divided in teams of two. We then randomly assigned groups to one of four treatments along two dimensions. First, team members either worked in the same or in different offices. Social interaction between team members was thus only possible when they shared an office. Other peer effects, however, were kept constant because workers shared an office with another worker in all treatments. Second, workers faced either team or relative incentives. Consequently, effort either imposed a positive or a negative externality on the team member's income. However, both the absolute size of the externality and the expected income were identical in all treatments. In order to control for skill heterogeneity, all applicants additionally completed a typing speed test at the end of the online application process.

We find that social interaction had no effect on work performance under team incentives. Further investigation reveals that workers internalized the positive externality irrespective of social interaction. Thus, team incentives did not give rise to a serious free riding problem. In contrast, social interaction drastically reduced work performance under relative incentives. This finding suggests that social interaction facilitated collu-

[1] Effort externalities can be inherent in the production process. For example, supermarket customers typically choose the register with the shortest waiting line. Thus, for a given number of customers, if one cashier works at higher pace, other cashiers will have a lower workload (Mas and Moretti, 2009). Effort externalities can also arise from the incentive schemes. For instance, promotion opportunities produce negative spillovers between workers because each worker's effort adversely affects the coworkers' probability of promotion (Holmström and Milgrom, 1990).

[2] We speak of "collusion" when implicit agreements harm the firm and "cooperation" when they help.

sion, and thus, the internalization of negative externalities. The establishment of collusive agreements, however, depends on within-team skill heterogeneity because potential gains from collusion are smaller for high-ability than for low-ability workers. We observe that social interaction reduced within-team performance differences for team members with similar skills. By contrast, collusive agreements were increasingly difficult the more team members differed with regard to their skills.

This study contributes to several strands of the literature. First, dozens of laboratory studies report that communication enhances cooperation in social dilemmas (Ledyard, 1995; Sally, 1995; Ostrom, 2006, for overviews). In a meta-analysis, Bicchieri and Lev-On (2007) compare the effectiveness of various communication channels and find that richer media (e.g. video conferences) achieve higher cooperation levels than thinner communication channels (e.g. chat). They conclude that richer media reveal more information about the counterpart and therefore simplify the establishment of cooperative agreements.[3] While lab experiments are characterized by a high degree of control, they are also embedded in highly stylized environments. For example, effort provision is typically not mentally or physically exhausting but simply a series of monetary transfers. The extent to which the results from laboratory experiments can be generalized to the field is thus unclear (Falk and Heckman, 2009; Levitt and List, 2007). Our field experiment allowed us to observe subjects in a more natural environment, while still maintaining a high level of experimental control. Moreover, subjects in lab experiments are aware that the experimenter scrutinizes their behavior. This may give raise to confounding demand effects (Zizzo, forthcoming). The workers in our study were not aware that they were participating in an experiment.

Second, two recent non-experimental field studies suggest that mutual monitoring allows workers to internalize effort externalities. Bandiera et al. (2005) find substantial productivity gains after a fruit farm switched from relative to individual piece rates. They observed no such gains, however, when fruit pickers had more difficulties monitoring each other. Bandiera et al. (2005) conclude that fruit pickers were unable to internalize the externalities in the absence of mutual monitoring. However, they were unable to analyze how the robustness of collusive agreements depends on skill heterogeneity. Mas and

[3]In a more related context, Harbring (2006) finds either enhanced cooperation or collusion, depending on the situation, when participants were allowed to chat with each other throughout the entire experiment. In contrast to our study, Harbring (2006) finds that communication is effective not only when negative but also when positive externalities are in place. The positive and negative externalities, however, were not comparable in size in her study.

Moretti (2009) examine scanner data from supermarket cash registers and find productivity spillovers among cashiers in the presence of positive externalities. These spillovers, however, only occurred in the line of vision of highly productive cashiers. Mas and Moretti (2009) conclude that monitoring is essential for internalizing positive externalities. We extend this literature by measuring social interaction effects in the presence of either positive or negative effort externalities in the same work environment.

Third, this study also complements the extensive management and psychology literature studying the effects of office arrangements on workers and their behavior. Scholars have shown, for example, that moving from closed to open offices increases stress (Wineman, 1986) and decreases job satisfaction (Oldham and Brass, 1979). Our field experiment adds to this literature by demonstrating how office arrangements affect work performance under different types of incentive schemes.

Finally, the results support Fehr and Falk's (2002) conjecture that "[...] under tournament incentives peer pressure against high performers will develop because high effort constitutes a negative externality for the competing workers". Our findings also provide an explanation for Lazear's (1989) observation that many firms avoid paying their workers based on tournament incentives, despite their potential to increase firm productivity considerably (Lazear and Rosen, 1981).

3.2 Experiment

3.2.1 Economic Environment

The books in a German university library needed to be electronically catalogued in March 2009. This provided us with the opportunity to conduct a field experiment. For this purpose, the university supplied four offices with two identical workstations in each office.[4]

3.2.2 Recruitment

We recruited workers three weeks prior to the experiment. The job was advertised on a job search website and with posters around the campus and in student dorms. Workers were offered a one-time job, limited to exactly four hours, at a wage of about €11 per hour. Applicants could show interest for the job with an online application form that

[4]Workstations were furnished with a desk, a notebook connected to the Internet, and an extra keyboard. See Figure 3.1 for an exemplary workstation.

ended with a five minute typing speed test. The online test was designed to mirror the future job task and serves as a measure of worker ability.[5] All hires were unaware they were participating in an experiment.[6]

3.2.3 Task

Upon arriving, workers were provided with an instruction sheet, and a research assistant explained the task to them.[7] Their task was to enter books into an electronic database.[8] They had to enter the title, author(s), publisher, ISBN number, and year of publication for each book. The database application recorded the exact time of each entry, allowing us to accurately measure performance over time without explicit monitoring.

Figure 3.1: Example of a Workstation

The figure displays a workstation used for the experiment. On each desk there was a notebook and piles of 250 randomly selected books in total.

[5]Applicants were hired on the basis of their availability without considering their performance in the typing speed test.

[6]Psychology students and applicants with insufficient local language skills were excluded from the applicant pool. We also made sure that none of the hires had participated in previous experiments at the local laboratory for economic experiments.

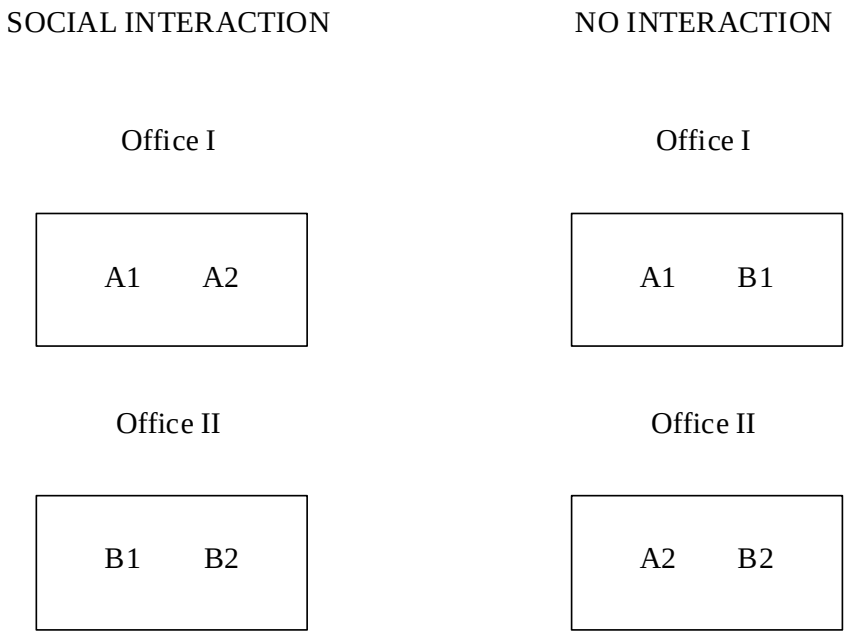
[7]The research assistants were instructed to follow a strict procedural protocol which included a detailed description of the communication with the workers.

[8]At each workstation there were piles of 250 randomly selected books in total. These piles were always of the same height and piles from different workstations were strictly separated, see Figure 3.1.

3.2.4 Treatments

Workers were invited to work in single sex groups of four and randomly allocated to teams of two. Each group of workers was randomly assigned to one of four treatments along two dimensions. First, the two team members either worked in the same or a separate office.[9] The physical separation of team members consequently prevented social interaction between them.[10] An important aspect of our design is that even in those treatments when team members were in a separate office, they shared this office with another person, as shown in Figure 3.2. Other peer effects, such as knowledge spillovers or distractions due to the presence of a peer, are therefore constant across treatments.

Figure 3.2: Office Arrangements



The figure illustrates how the office arrangement determined whether social interaction between team members was possible or not. Teams are denoted by A and B, and team members by 1 and 2.

Second, workers faced either team or relative incentives. Under team incentives – when effort provides a positive externality to others – workers earned an hourly wage of €3.50 and a piece rate of €0.10 per entry of the whole team.[11] Under relative incentives

[9] If one of the four workers did not show up, only one team could be formed. Two team members were then assigned the same office, while the remaining worker was assigned to work alone in an office.

[10] The research assistants were instructed to keep minutes on whether workers went into the other team member's office. This never happened, however.

[11] We used a pilot study to calibrate the fixed component of the wage under team incentives so that

– when effort imposes a negative externality on others – workers earned an hourly wage of €11.00 and a piece rate that depended on the performance gap between team members: workers gained (lost) €0.10 for each book they entered more (less) than their opponent team member. The incentive schemes share two common features: both the absolute size of the externality (€0.10 per entry) and the expected wage (€11.00 per hour, taking the income from the incentive scheme into account) were identical.[12] The research assistants made sure that every worker understood the payment scheme and also provided them with a handout illustrating the incentive scheme in force.

3.3 Behavioral Predictions

If team members are spatially separated, social interaction between them is inhibited. This implies that selfish workers maximize their private surplus without taking the externality they impose on the team member's income into account. Consequently, under either team or relative incentives, workers will enter books up to the point where their marginal cost of effort equals their private marginal revenue.

If team members share the same office, social interaction between them is possible. This allows team members to maximize their joint surplus by establishing implicit effort agreements that take the externalities into account. Agreements are sustainable if both team members achieve a higher private surplus by sticking to them than by unilateral deviation. We assume that the other team member punishes defection.[13] The extent to which team members are able to maximize the joint surplus is determined by the punishment possibilities. If the punishment threat is sufficiently severe, team members internalize the externality, at least to some degree.

We therefore predict that social interaction has a positive impact on work performance when team incentives are in place. In other words, social interaction mitigates the free riding problem and fosters cooperation. In contrast, we should observe the opposite, namely a reduction in work performance, when workers face relative incentives. In this

the expected wage corresponded to the projected wage of €11.00 per hour.

[12]Single workers received either an hourly wage of €3.50 and an individual piece rate of €0.10 per entry if others were paid according to team incentives, or they earned an hourly wage of €11.00 if the others were paid according to relative incentives.

[13]We assume that punishment for both team members is of equal size and costless, such as verbal punishment. Cooper and Kühn (2009), for example, show the effectiveness of verbal punishment in a laboratory experiment on collusion. Relaxing this assumption by allowing for costly punishment would not change the behavioral mechanism explained here. However, repeated game incentives or social preferences would be needed in order to make punishment a credible threat.

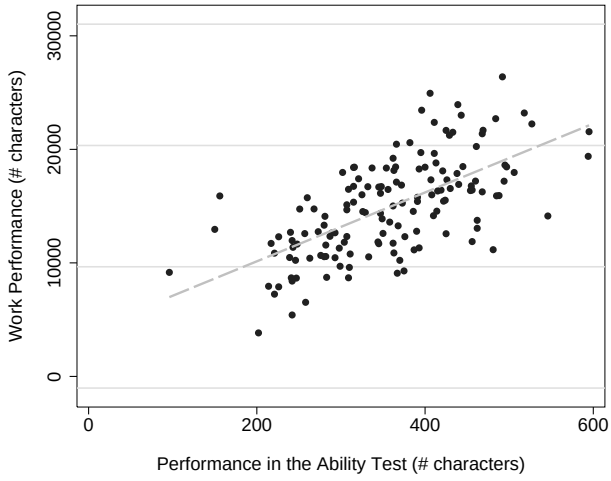
case, social interaction eliminates competition through collusion.

3.4 Results

3.4.1 Descriptive Statistics

Our sample consists of 146 workers in 73 teams.[14] Workers were predominantly female (74 percent) and in their mid-twenties (25 years on average). Entering books into a database is a very simple task. Nevertheless, workers' abilities vary substantially. Comparing the top and bottom performance decile in the five minute typing speed test reveals that top performers were able to enter twice as much as low performers. Performance in this online test is highly correlated with actual work performance, as indicated in Figure 3.3.[15] Work performance is measured by the number of typed characters in correct book entries.[16] Our measure of ability thus allows us to control for skill heterogeneity.

Figure 3.3: Correlation of Ability and Work Performance



[14]Under team (relative) incentives, 9 (3) workers could not be allocated to a team because one of the four workers did not show up.

[15]The spearman rank correlation between test and work performance is 0.66 and highly significant ($p < 0.0001$).

[16]A research assistant searched for incorrectly entered ISBN numbers and spelling mistakes in the book titles using an automatic spell check program.

3.4.2 Randomization Check

Table 3.1 shows treatment averages for all variables used in this study and tests whether explanatory variables are equally distributed across treatments. Worker characteristics appear balanced across the four treatments. There are no significant differences with respect to test performance ($p = 0.46$, Kruskal-Wallis test) and age ($p = 0.17$, Kruskal-Wallis test). There is, however, one exception: women are less likely to be in the team incentive treatment without social interaction ($p < 0.01$, χ^2 test). Work conditions are less balanced because only one treatment per day was conducted.[17] Afternoon shifts ($p = 0.49$, χ^2 test) and the four offices (p-values between 0.45 and 0.84, χ^2 test) are perfectly balanced. Conversely, weekdays ($p < 0.01$, χ^2 test), temperature ($p < 0.01$, Kruskal-Wallis test), and sunshine hours ($p = 0.08$, Kruskal-Wallis test) are not balanced across treatments. Thus, all regression estimations in the analysis include a specification that controls for worker characteristics and work conditions.

Table 3.1: Randomization Check

	Incentives				
	TEAM		RELATIVE		
Interaction	SOCIAL	NO	SOCIAL	NO	p-Value
Ability (typing speed test)	347.2	363.4	348.7	372.2	0.46
Age	24.3	23.7	24.6	26.1	0.17
Female (in %)	67	56	82	90	<0.01
Afternoon	0.3	0.4	0.4	0.4	0.49
Monday	0.6	0.0	0.0	0.0	<0.01
Tuesday	0.2	0.4	0.1	0.2	<0.01
Wednesday	0.1	0.2	0.5	0.1	<0.01
Thursday	0.1	0.3	0.1	0.5	<0.01
Friday	0.0	0.0	0.4	0.2	<0.01
Office 1	0.3	0.3	0.4	0.4	0.84
Office 2	0.3	0.3	0.2	0.4	0.45
Office 3	0.2	0.2	0.2	0.1	0.55
Office 4	0.2	0.2	0.2	0.1	0.77
Temperature ($°C$)	15.6	19.4	17.0	17.5	<0.01
Sunshine (in hours)	5.9	8.3	6.3	7.7	0.08

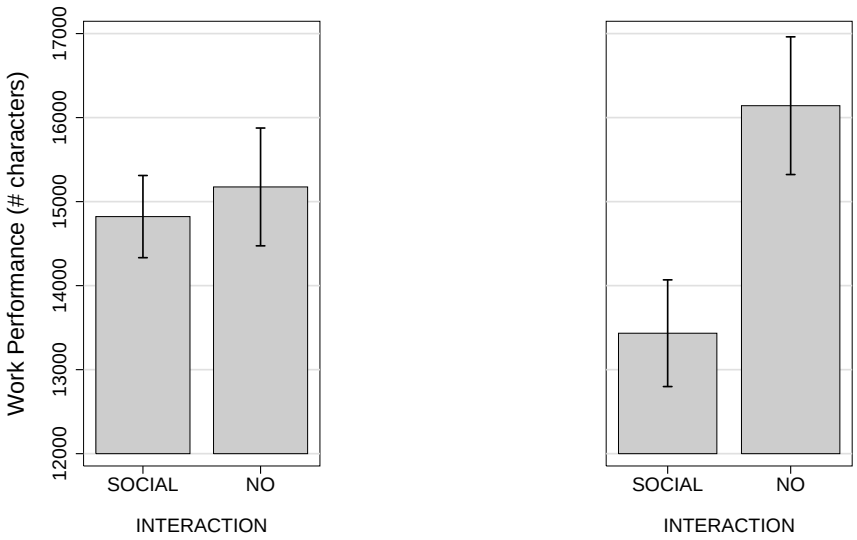
The last column contains p-values (χ^2 squared test for binary variables and Kruskal-Wallis test of equality of populations for non-binary variables) for the null hypothesis of perfect randomization.

[17] This was done in order to prevent confusion about treatment assignment among the research assistants.

3.4.3 Social Interaction and Team Incentives

We first examine the impact of social interaction on total work performance under team incentives. Figure 3.4.3 illustrates average work performance, including standard errors, across treatments.[18] Panel (a) of this figure shows that workers were about equally productive under team incentives, irrespective of social interaction. A non-parametric test cannot reject the null hypothesis of equal work performance ($p = 0.90$, Mann-Whitney U).[19]

Figure 3.4: Impact of Social Interaction on Work Performance



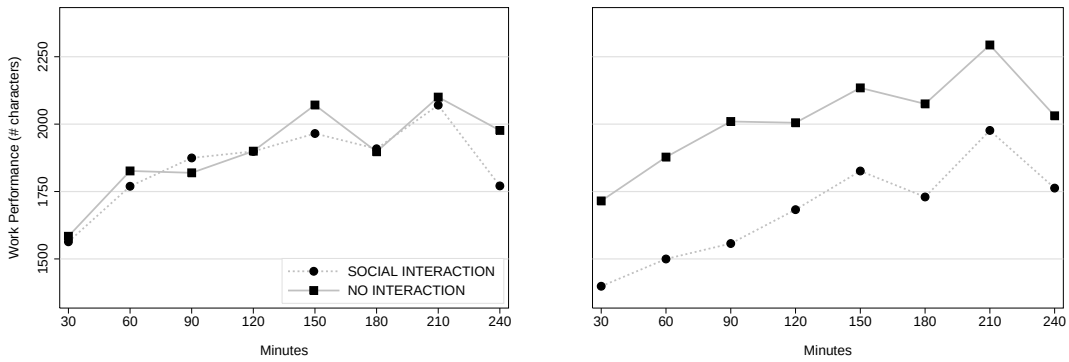
(a) TEAM INCENTIVES (b) RELATIVE INCENTIVES

In order to analyze whether there is a short lived social interaction effect, we partition the working time into 30-minute time periods, as shown in Figure 3.4.3. Panel (a) of this figure visualizes the time course of work performance across the two team incentive treatments. The figure indicates that work performance in both treatments is almost identical over the entire working time. Another striking pattern is that both treatments show a similar upward trend in work performance over time, suggesting that workers gained experience in performing the task.

[18]Standard errors are clustered by worker pairs sharing the same office to account for dependence.

[19]We use average performance per office to use statistically independent observations. All p-values in the analysis are two-sided.

Figure 3.5: Impact of Social Interaction on Work Performance over Time



(a) TEAM INCENTIVES (b) RELATIVE INCENTIVES

To test these data patterns more formally and to control for observable differences in worker characteristics and work conditions, we estimate a series of random effect models of the following form

$$\log(y_{ikt}) = \alpha_i + \beta_0 + \beta_1 S_i + \beta_2 P_t + \beta_3 S_i \times P_t + \beta_4 \log(A_i) + \beta_5 \vec{X}_i + \epsilon_{ikt}, \tag{3.1}$$

where $\log(y_{ikt})$ denotes the logarithm of the work performance of worker i in office k and period t; α_i is the individual random effect, the constant β_0 captures average initial performance in the omitted category, S_i is a dummy indicating whether social interaction with the team member was possible, P_t is a count variable for 30-minute periods ranging from zero to seven, $S_i \times P_t$ captures the interaction between social interaction and the time trend, $\log(A_i)$ is the logarithm of the performance in the typing speed test, and $\vec{X}_i$ is a vector of control variables for worker characteristics (age and gender) and work conditions (daytime, weekday, office, temperature, and sunshine hours); finally, ϵ_{ikt} is the idiosyncratic error term, which we allow to be correlated within worker pairs sharing the same office.

Table 3.2 shows the coefficient estimates for equation (3.1). We find that social interaction had no effect on work performance under team incentives. The coefficients of both social interaction and its interaction with the time trend are close to zero and therefore insignificant in all columns. The coefficient of time period is consistently positive and highly significant, suggesting a learning effect. Column (2) includes the performance in the ability test as a control variable. Skills and work performance are strongly correlated:

a 1 percent performance increase in the ability test was associated with a 0.6 percent higher work performance. All estimates are robust to the inclusion of additional controls, as shown in Column (3).

Table 3.2: Impact of Social Interaction on Work Performance under Team Incentives

Dependent variable: Log(# characters)	(1)	(2)	(3)
SOCIAL INTERACTION	0.014	0.046	0.054
	(0.067)	(0.050)	(0.080)
Time period	0.029***	0.029***	0.028***
	(0.005)	(0.005)	(0.005)
SOCIAL INTERACTION × time period	–0.008	–0.008	–0.007
	(0.007)	(0.007)	(0.007)
Log(ability)		0.644***	0.690***
		(0.098)	(0.104)
Constant (omitted: NO INTERACTION)	7.399***	3.623***	3.589***
	(0.051)	(0.578)	(0.686)
Controls	No	No	Yes
N	576	576	568[a]

Random effect model estimates. Standard errors, adjusted for clustering on worker pairs working in the same office, in parentheses. Dependent variable: number of hourly characters from correct entries. [a] One worker did not state her date of birth. Level of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Result 1 *Social interaction had no effect on work performance under team incentives.*

Why did social interaction not foster cooperation when workers faced team incentives? A plausible explanation is that workers internalized the positive externality even in the absence of social interaction. To test this hypothesis, we use data from single workers who were paid an individual piece rate of €0.10 per entry. The only difference between individual and team incentives is that the individual piece rate did not create an externality. Thus, if workers managed to internalize the positive externality, we should observe a higher work performance under team than under individual incentives.

Table 3.3 reports coefficient estimates from a random effect model that compares the work performance of single workers and the performance of those workers with team incentives and who could not interact with their team member.[20] Column (1) shows

[20]Because only 9 workers were subject to individual incentives, we do not have enough variation to control for worker characteristics and work conditions in the regression analysis.

that despite the lack of social interaction, work performance is 23 percent higher under team than under individual incentives. This performance difference, however, drops to 10 percent and looses significance when we control for performance in the ability test, as shown in Column (2).

Table 3.3: Individual vs. Team Incentives without Social Interaction

Dependent variable: Log(# characters)	(1)	(2)
NO INTERACTION × TEAM INCENTIVES	0.232**	0.095
	(0.098)	(0.094)
Time period	0.028**	0.029**
	(0.011)	(0.011)
NO INTERACTION × TEAM INCENTIVES × time period	0.001	0.001
	(0.013)	(0.013)
Log(ability)		0.784***
		(0.097)
Constant (omitted: INDIVIDUAL INCENTIVES)	7.168***	2.704***
	(0.083)	(0.565)
Controls	No	No
N	355	355

Random effect model estimates. Standard errors, adjusted for clustering on worker pairs working in the same office, in parentheses. Dependent variable: number of hourly characters from correct entries. Sample: workers who were paid according to individual or team incentives. In case of team incentives, team members worked in separate offices. Level of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Result 2 *Workers tended to internalize the positive externality even in the absence of social interaction.*

3.4.4 Social Interaction and Relative Incentives

We now analyze the impact of social interaction on work performance when relative incentives were in place. Panel (b) of Figure 3.4.3 shows that social interaction was detrimental to total work performance when effort imposed a negative externality. Social interaction reduced work performance by an average of 17 percent, and this difference is significant as indicated by a non-parametric test ($p = 0.03$, Mann-Whitney U).

We again used the partitioning of the work duration into 30-minute time periods in order to examine how the treatment effect evolved over time. Panel (b) of Figure 3.4.3

indicates that social interaction reduced work performance immediately, and that this reduction persisted over the entire working time.

We complement this analysis and estimate a series of random effect models using equation (3.1) for the sample of workers who faced relative incentives. All three model specifications in Table 3.4 reveal that social interaction caused a significant drop in work performance under relative incentives. The point estimate varies between –0.21 and –0.28, meaning that initial work performance was between 21 and 28 percent lower in the presence of social interaction. We only find a small and insignificant interaction effect between social interaction and the time trend, indicating that the treatment effect persisted over time. In addition, the temporal upward trend of work performance is similar to the treatments with team incentives. Columns (2) and (3) add the performance in the typing speed test as control variable. Skills are again strongly correlated with work performance. The inclusion of additional controls, as shown in Column (3), does not qualitatively change the insights gained from the baseline specification.

Table 3.4: Impact of Social Interaction on Work Performance under Relative Incentives

Dependent variable: Log(# characters)	(1)	(2)	(3)
SOCIAL INTERACTION	–0.253***	–0.216**	–0.304***
	(0.096)	(0.086)	(0.076)
Time period	0.028***	0.028***	0.028***
	(0.005)	(0.005)	(0.005)
SOCIAL INTERACTION × time period	0.014	0.014	0.014
	(0.013)	(0.013)	(0.013)
Log(ability)		0.597***	0.512***
		(0.127)	(0.106)
Constant (omitted: NO INTERACTION)	7.464***	3.954***	2.094*
	(0.057)	(0.744)	(1.097)
Controls	No	No	Yes
N	591	591	591

Random effect model estimates. Standard errors, adjusted for clustering on worker pairs working in the same office, in parentheses. Dependent variable: number of hourly characters from correct entries. Level of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Result 3 *Social interaction significantly reduced work performance under relative incentives.*

These results are consistent with the notion that team members established implicit collusive agreements to eliminate competition between them. A more direct test of collision is to look whether within-team performance differences are smaller. We therefore estimate a series of OLS regression models at the team level which are specified as follows

$$\log(|y_i - y_{-i}|_j) = \gamma_0 + \gamma_1 S_i + \gamma_2 \log(|A_i - A_{-i}|_j) + \gamma_3 S_i \times \log(|A_i - A_{-i}|_j) + \gamma_4 \vec{X_j} + \epsilon_j, \tag{3.2}$$

where $\log(|y_i - y_{-i}|_j)$ denotes the logarithm of the absolute difference in work performance between workers i and $-i$ in team j, the constant γ_0 captures average work performance differences in the omitted category, S_i is a dummy indicating whether social interaction between team members was possible, $\log(|A_i - A_{-i}|_j)$ represents the logarithm of the absolute performance difference in the ability test between workers i and $-i$ in team j, $S_i \times \log(|A_i - A_{-i}|_j)$ denotes the interaction between social interaction and the absolute performance difference in the ability test, $\vec{X_j}$ is a vector of control variables for team characteristics (mean age and gender) and work conditions (daytime, weekday, temperature, and sunshine hours); finally, ϵ_j is the idiosyncratic error term, which we allow to be correlated within worker pairs sharing the same office.

Table 3.5 presents the coefficient estimates for equation (3.2). As shown in Column (1), social interaction reduced within team-performance differences, but the coefficient estimate is small and insignificant. A plausible explanation is that large ability differences within teams made collusive agreements difficult to establish. This is because high-ability workers could easily outperform their team member, and thus, had a higher temptation to renege on collusive agreements. In order to analyze heterogeneity in the treatment effect, we include an interaction term for within-team ability differences and social interaction in Column (2). The coefficient of social interaction is now negative and significant. This finding suggests that social interaction reduced within-team performance differences for teams with homogeneous skills. By contrast, within-team performance differences increase the more team members differed with respect to their skills, as indicated by a positive and significant coefficient of the interaction term. The inclusion of additional controls, as shown in Column (3), does not qualitatively change the results.

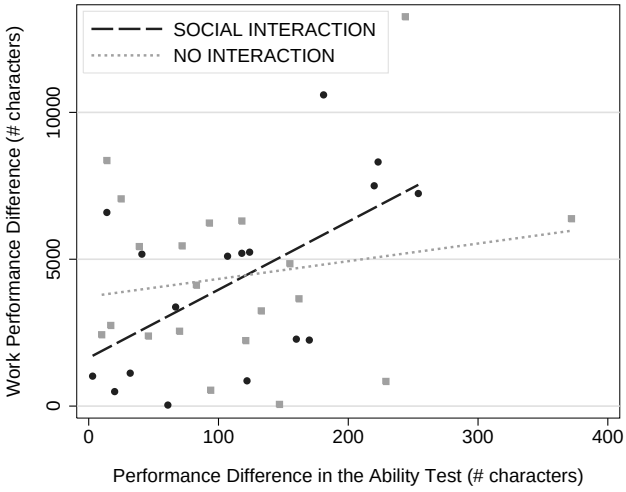
Table 3.5: Performance Differences within Teams under Relative Incentives

Dependent variable: Log(|Δ # characters|)	(1)	(2)	(3)
SOCIAL INTERACTION	-0.161	-2.721**	-2.768**
	(0.448)	(1.280)	(1.250)
Log(|Δ ability|)	0.166	-0.159	-0.182
	(0.138)	(0.228)	(0.380)
SOCIAL INTERACTION× log(|Δ ability|)		0.592*	0.810**
		(0.296)	(0.348)
Constant (omitted: NO INTERACTION)	7.280***	8.696***	11.841
	(0.572)	(0.837)	(11.828)
Controls	No	No	Yes
N	37	37	37

OLS estimates. Standard errors, adjusted for clustering on worker pairs working in the same office, in parentheses. Dependent variable: absolute within-team difference of the number of hourly characters from correct entries. Level of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure 3.6 illustrates the heterogeneity in the treatment effect. The figure plots absolute within-team performance differences both in the ability test and on the job. Team members with similar skills had a more similar work performance in the presence of social interaction, while the opposite is true for those who differed strongly with respect to their skills.

Figure 3.6: Within-Team Performance and Ability Differences



Result 4 *Under relative incentives, social interaction significantly reduced within team-performance differences for homogeneous teams, but did not promote collusion in heterogeneous teams.*

3.5 Conclusion

We conducted a field experiment to examine the impact of social interaction on work performance in the presence of either positive or negative effort externalities. We employed workers to catalogue the books in a library. Hires were invited in groups of four and randomly allocated to teams of two. The two team members were either assigned to work in the same or a separate office. Social interaction between team members was thus only possible if they shared the same office. Other peer effects, however, were kept constant across treatments because workers always shared the office with another person. Workers were then paid either according to team or relative incentives. Under team incentives, workers' effort imposed a positive externality on team member's pay, while it imposed a negative externality under relative incentives.

We show that social interaction had no impact on work performance under team incentives. Further analysis reveals that workers internalized the positive externality even without social interaction. By contrast, social interaction considerably reduced work performance under relative incentives. We further find that social interaction significantly lowered performance differences between team members with similar skills. Both findings provide evidence that social interaction led to collusion when workers faced negative externalities. However, we find that collusion was increasingly difficult the more team members differed in skills. This result illustrates the limits of collusion.

An open question is whether side-payments would have allowed workers in heterogeneous teams to overcome their difficulty to collude. Workers with a lower ability than their team member had more to gain from collusion and therefore could have compensated their team member for forgone earnings. We only observe two cases of side-payments at the end of work, and surprisingly, in the opposite direction: top performers of a team offered to share part of their earnings with the slower team member. This observation suggests that the motivation behind the side-payments were fairness concerns rather than an attempt to sustain collusion.

If side-payments are nevertheless an issue, firms could theoretically set discriminatory wages that inhibit the effectiveness of such transfers (Ishiguro, 2004). They could offer

higher-powered incentives to the high-ability workers so that their foregone earnings of collusion are too large to be compensated.

Finally, Becker and Sims (2001) show that office plans largely determine the intensity of social interaction at the workplace. For example, open office designs were adopted to maximize work-related communication between workers by removing the physical barriers that hinder the workflow (Pile, 1976). Our results underscore the importance of taking the particulars of office arrangements for the design of optimal incentive schemes into account.

Appendices

Appendix to Chapter 1

Treatment Postcards

DAS NEUSTE AM ABEND
heute

Lieber Promoter,

Ihr Standort ist für uns heute sehr wichtig. Gehen Sie bitte besonders aktiv auf die Passanten zu. Dafür bezahlen wir Ihnen auch CHF 5.- mehr pro Stunde.

Die exakte Anzahl der verteilten Zeitungen ist sehr wichtig. Behalten Sie einen Bändel (1 Stapel = 100 Zeitungen) pro vollständig verteilten Stapel. Zählen Sie zudem die verteilten Zeitungen des letzten, angefangenen Stapels. Schicken Sie **am Ende der Schicht ein Antwort-SMS mit Namen, Standort und Gesamtzahl verteilter Zeitungen** an **00447624802675.**
Ihr heute-Team

DAS NEUSTE AM ABEND
heute

Lieber Promoter,

Die exakte Anzahl der verteilten Zeitungen ist sehr wichtig. Behalten Sie einen Bändel (1 Stapel = 100 Zeitungen) pro vollständig verteilten Stapel. Zählen Sie zudem die verteilten Zeitungen des letzten, angefangenen Stapels. Schicken Sie **am Ende der Schicht ein Antwort-SMS mit Namen, Standort und Gesamtzahl verteilter Zeitungen** an **00447624802675.**
Ihr heute-Team

Feedback Form

Heute... das Neuste am Abend – PROMOTOREN - FEEDBACK

Lieber Promotor

Herzlichen Dank für Deinen Einsatz an der Promotion für *heute*. Zum Abschluss dieser Aktion möchten wir gerne Dein Feedback dazu haben. Bitte sende uns dieses ausgefüllte Feedbackblatt bis spätestens 29. September 2006 zu.

Die Bezahlung von CHF 22.-/Stunde für diese Tätigkeit empfand ich als …

☐	☐	☐	☐	☐
knauserig		angemessen		grosszügig

Das genaue Zählen der Anzahl verteilter *heute*-Zeitungen empfand ich als …

☐	☐	☐	☐	☐
nicht anstrengend		anstrengend		sehr anstrengend

Die Spezialaufgabe (besonders aktiv auf Leute zuzugehen) empfand ich als …

☐	☐	☐	☐	☐
nicht anstrengend		anstrengend		sehr anstrengend

Die Lohnerhöhung von CHF 5.-/Stunde für die Spezialaufgabe empfand ich als …

☐	☐	☐	☐	☐
unangemessen		fair		grosszügig

Wie häufig möchtest Du weiter für [*Firma*] im Einsatz stehen?

☐	☐	☐	☐	☐
nicht mehr	nur abends nach 17.00 Uhr	am Wochenende	MO-FR unter der Woche	auf Anfrage

Welche Nationalität hast du? _____

Arbeitsbewilligung: ☐ B ☐ C ☐ L ☐ Andere: _____

Haben Freunde von dir bei der *heute*-Promotion mitgearbeitet? Falls Ja, zähle diese der Reihe nach auf (1 = bester Freund, 2 = zweitbester Freund etc.)

1 _____
2 _____
3 _____
4 _____
5 _____
6 _____
7 _____

Allgemeine Beobachtungen und Kommentare:

_____
_____
_____

Follow-Up Survey

Universität Zürich
Institut für Empirische Wirtschaftsforschung

17. Oktober 2006

Wissenschaftliche Studie über Teilzeitbeschäftigung in der Schweiz

[*Anrede*]

Mit freundlicher Genehmigung der [*Firma*] wurden Sie von der Universität Zürich zusammen mit vielen anderen Teilzeitbeschäftigten aus der Schweiz ausgewählt, um an einer wissenschaftlichen Studie teilzunehmen. Die Studie besteht aus einem Entscheidungsteil (gelbe Blätter) – hier können Sie Entscheidungen treffen – und einem Fragebogen (rosa Blätter). **Durch Ihre Teilnahme an dieser Studie helfen Sie uns, die Situation von Teilzeitbeschäftigten besser zu verstehen.** Wir bitten Sie deshalb, an dieser Studie teilzunehmen. **Ausserdem können Sie durch Ihre Teilnahme an dieser Studie – das sorgfältige und vollständige Ausfüllen des Entscheidungsteils und des Fragebogens – bis zu 25 Franken verdienen.**

Lesen Sie deshalb bitte als erstes die Beschreibung des Entscheidungsteils auf den Seiten 2 bis 4 durch. Füllen Sie anschliessend die gelben Entscheidungsblätter und den rosa Fragebogen aus. Zum Schluss stecken Sie bitte die Entscheidungsblätter und den Fragebogen in den bereits frankierten Antwort-Umschlag und schicken es bis **Montag 30. Oktober 2006 (Poststempel)** an uns zurück.

Sobald wir Ihren Antwort-Umschlag (mit Entscheidungsblättern und Fragebogen) fristgerecht erhalten haben, schicken wir Ihnen per Post den Geldbetrag, den Sie verdient haben, in bar zu. Alle Daten werden ausschliesslich für wissenschaftliche Zwecke verwendet und nur in anonymisierter Form ausgewertet. Wir garantieren Ihnen, dass niemand, weder die [*Firma*] noch irgendwelche andere Firmen, Zugang zu diesen Daten erhalten.

Falls Sie Fragen zur Studie haben (z.B. zum Ausfüllen der Entscheidungsblätter), erteilen wir Ihnen gerne Auskunft unter der Telefonnummer [*Telefonnummer*], oder schreiben Sie uns eine E-Mail [*E-Mail*].

Vielen Dank für Ihre Teilnahme!

Mit freundlichen Grüssen
Institut für Empirische Wirtschaftsforschung

IHRE ENTSCHEIDUNGSSITUATION

A. WORUM GEHT ES?

Es geht in dieser Studie um die **Aufteilung eines Geldbetrages zwischen 2 Personen** und um das **Belohnen und Bestrafen.** Sie sind in dieser Studie mit einer zufällig ausgewählten **anderen Person zusammen in einer Zweiergruppe**.
Zuerst muss die andere Person entscheiden, wie sie einen Geldbetrag von 24 Franken zwischen Ihnen beiden aufteilt. Dann haben Sie die Möglichkeit, die Entscheidung der anderen Person zu bewerten, indem Sie der anderen Person Bonuspunkte oder Minuspunkte zuweisen. Sie können aber auch gar keine Punkte zuweisen.

Wir garantieren die Anonymität aller Entscheidungen. Das bedeutet, dass Sie die Identität der anderen Person nie erfahren. Diese erfährt auch nie etwas über Ihre Identität. Wir garantieren auch dafür, dass alle Geldbeträge, die aufgrund der Entscheidungen verdient werden, an Sie und die andere Person ausbezahlt werden.

Wie kann die andere Person die 24 Franken aufteilen?

Die andere Person hat 3 verschiedene Möglichkeiten, die 24 Franken aufzuteilen. Sie kann

- **18 Franken** für sich selbst behalten und Ihnen **6 Franken** geben, oder
- **12 Franken** für sich selbst behalten und Ihnen **12 Franken** geben, oder
- **6 Franken** für sich selbst behalten und Ihnen **18 Franken** geben.

Worüber können Sie entscheiden?

Sie können der **anderen Person bis zu 2 Bonus- oder Minuspunkte zuweisen**, oder Sie können nichts unternehmen, d.h. **keine Punkte** zuweisen.

Bonuspunkte: Jeder Bonuspunkt den Sie der anderen Person zuweisen, erhöht deren Auszahlung – relativ zum Aufteilungsvorschlag – um 6 Franken und kostet Sie 2 Franken. Wenn Sie also beispielsweise 2 Bonuspunkte zuweisen, dann erhöht sich die Auszahlung der anderen Person um 12 Franken und es kostet Sie 4 Franken. Mit Bonuspunkten können Sie also die andere Person belohnen.

Minuspunkte: Jeder Minuspunkt den Sie der anderen Person zuweisen, verringert deren Auszahlung – relativ zum Aufteilungsvorschlag – um 6 Franken und kostet Sie 2 Franken. Wenn Sie also beispielsweise 2 Minuspunkte zuweisen, dann verringert sich die Auszahlung der anderen Person um 12 Franken und es kostet Sie 4 Franken. Mit Minuspunkten können sie also die andere Person bestrafen.

Keine Punkte: In diesem Fall erhält die andere Person keine Bonus- und keine Minuspunkte. Sie und die andere Person verdienen dann soviel wie im Aufteilungsvorschlag vorgesehen ist.

Durch die Vergabe von Bonus- oder Minuspunkten können Sie also die Auszahlungen - relativ zum Aufteilungsvorschlag – verändern. Wenn Sie keine Punkte zuweisen, werden die Auszahlungen nicht verändert.

B. EIN PAAR BEISPIELE

Hier finden Sie noch ein paar Beispiele zur Berechnung der Geldbeträge.

Beispiel 1: Die andere Person behält 18 Franken für sich selbst und gibt Ihnen 6 Franken.

- Wenn Sie dann der anderen Person beispielsweise 2 Minuspunkte zuweisen, dann verringert dies die Auszahlung der anderen Person um 12 Franken und es kostet Sie 4 Franken.
  Die andere Person hat jetzt neu 18 – 12 = 6 Franken und Sie verdienen 6 – 4 = 2 Franken.

- Wenn Sie hingegen der anderen Person beispielsweise 2 Bonuspunkte zuweisen, dann erhöht dies die Auszahlung der anderen Person um 12 Franken und kostet Sie 4 Franken.
  Die andere Person hat jetzt neu 18 + 12 = 30 Franken und Sie verdienen 6 – 4 = 2 Franken.

- Wenn Sie keine Punkte vergeben, dann erhält die andere Person 18 Franken und Sie verdienen 6 Franken.

Beispiel 2: Die andere Person behält 6 Franken für sich selbst und gibt Ihnen 18 Franken.

- Wenn Sie dann der anderen Person beispielsweise 1 Bonuspunkt zuweisen, dann erhöht dies die Auszahlung der anderen Person um 6 Franken und kostet Sie 2 Franken.
  Die andere Person hat jetzt neu 6 + 6 = 12 Franken und Sie verdienen 18 – 2 = 16 Franken.

- Wenn Sie hingegen der anderen Person beispielsweise 1 Minuspunkt zuweisen, dann verringert dies die Auszahlung der anderen Person um 6 Franken und kostet Sie 2 Franken.
  Die andere Person hat jetzt neu 6 – 6 = 0 Franken und Sie verdienen 18 – 2 = 16 Franken.

- Wenn Sie keine Punkte vergeben, dann erhält die andere Person 6 Franken und Sie verdienen 18 Franken.

C. DER GENAUE ABLAUF DER EREIGNISSE

Wir bitten Sie, **für alle 3 möglichen Aufteilungen,** welche die andere Person vorschlagen kann, **festzulegen, wie viel Bonus- bzw. Minuspunkte Sie vergeben** oder ob Sie **keine Punkte** vergeben. Sie müssen also für **jeden** der drei folgenden Fälle eine Entscheidung treffen:

- **Fall 1:** Die **andere Person behält 18 Franken** für sich selbst und **gibt Ihnen 6 Franken**
- **Fall 2:** Die **andere Person behält 12 Franken** für sich selbst und **gibt Ihnen 12 Franken**
- **Fall 3:** Die **andere Person behält 6 Franken** für sich selbst und **gibt Ihnen 18 Franken**

Welcher Fall für die Auszahlung relevant ist, hängt von der Entscheidung der anderen Person ab. Die Unterlagen für diese Studie werden an viele Person versandt und deshalb wissen wir derzeit noch nicht, welche Entscheidung die andere Person getroffen hat. Wenn aber die andere Person beispielsweise 12 Franken für sich selbst nimmt und 12 Franken an sie gibt, dann ist Fall 2 relevant.

Beachten Sie, dass es bei dieser Entscheidungssituation keine richtigen oder falschen Entscheide gibt. Für uns ist es wichtig, dass Sie Ihre Entscheidungen und Antworten vollständig und sorgfältig treffen. Bitte treffen Sie Ihre Entscheide alleine und besprechen Sie sich nicht mit anderen Personen, z.B. Freunden. Ihre Anonymität bleibt vollständig gewahrt.

D. HINWEISE

Wie setzt sich Ihr Gesamteinkommen aus dieser Studie zusammen?

Das Gesamteinkommen setzt sich einmal aus

(1) dem Einkommen aus dem Entscheidungsteil (bis zu 18 Franken) und

(2) einem Fixbetrag von 7 Franken für das vollständige Ausfüllen des Fragebogens zusammen.

Wie bekommen Sie Ihr Geld?
Sobald wir die Antwort-Umschläge von den Teilnehmerinnen und Teilnehmer erhalten haben, werden die Einkommen berechnet. Sie erhalten von uns das verdiente Geld per Post in bar zugesandt.

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ENTSCHEIDUNGSBLATT FÜR DEN FALL 1

Ihre Entscheidung für den Fall 1: Die andere Person behält **18 Franken** für sich selbst und gibt Ihnen **6 Franken**.				
Für diesen Fall vergebe ich folgende Punkte: (Bitte kreuzen Sie eines der unten angegebenen Kästchen an)				
2 Minuspunkte. ❑	1 Minuspunkt. ❑	0 Punkte. ❑	1 Bonuspunkt. ❑	2 Bonuspunkte. ❑

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ENTSCHEIDUNGSBLATT FÜR DEN FALL 2

Ihre Entscheidung für den Fall 2: Die andere Person behält **12 Franken** für sich selbst und gibt Ihnen **12 Franken**.				
Für diesen Fall vergebe ich folgende Punkte: (Bitte kreuzen Sie eines der unten angegebenen Kästchen an)				
2 Minuspunkte. ❑	1 Minuspunkt. ❑	0 Punkte. ❑	1 Bonuspunkt. ❑	2 Bonuspunkte. ❑

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ENTSCHEIDUNGSBLATT FÜR DEN FALL 3

Ihre Entscheidung für den Fall 3: Die andere Person behält **6 Franken** für sich selbst und gibt Ihnen **18 Franken**.				
Für diesen Fall vergebe ich folgende Punkte: (Bitte kreuzen Sie eines der unten angegebenen Kästchen an)				
2 Minuspunkte. ❑	1 Minuspunkt. ❑	0 Punkte. ❑	1 Bonuspunkt. ❑	2 Bonuspunkte. ❑

Haben Sie für alle 3 möglichen Aufteilungsvorschläge je ein Kästchen angekreuzt?

Dann geben Sie bitte in wenigen Stichworten an, welche Überlegungen hinter Ihren Entscheiden standen.

_ _

_ _

_ _

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FRAGEBOGEN

Zehn Fragen zu Ihrer Teilzeitarbeit im letzten halben Jahr.

Bitte beantworten Sie die unten stehenden Fragen in Bezug auf Ihre **Teilzeitjobs im letzten Jahr**. Haben Sie mehr wie drei Arbeitsverhältnisse ausgeübt, bitten wir Sie die Fragen in Hinsicht auf die 3 wichtigsten Arbeitsverhältnisse zu beantworten. Bei weniger wie drei Arbeitsverhältnissen können Sie die entsprechenden Felder für Arbeitgeber 2, respektive Arbeitgeber 3, leer lassen.

1) Nennen Sie Ihre Teilzeit-Arbeitgeber im letzten halben Jahr.

Arbeitgeber 1 **[*Firma*]** _.

Arbeitgeber 2 _.

Arbeitgeber 3 _.

2) Welche Tätigkeiten haben Sie ausgeübt?

Bei Arbeitgeber 1 habe ich als _ *gearbeitet.*

Bei Arbeitgeber 2 habe ich als _ *gearbeitet.*

Bei Arbeitgeber 3 habe ich als _ *gearbeitet.*

3) Wie viele Stunden haben Sie pro Woche beim jeweiligen Arbeitgeber gearbeitet?

Bei Arbeitgeber 1 habe ich _ _ _ _ _ *Stunden pro Woche gearbeitet.*

Bei Arbeitgeber 2 habe ich _ _ _ _ _ *Stunden pro Woche gearbeitet.*

Bei Arbeitgeber 3 habe ich _ _ _ _ _ *Stunden pro Woche gearbeitet.*

4) Wie viele Monate möchten Sie noch für Ihre derzeitigen Teilzeit-Arbeitgeber arbeiten?

Bei Arbeitgeber 1 möchte ich noch _ _ _ _ _ Monate weiterarbeiten.

Bei Arbeitgeber 2 möchte ich noch _ _ _ _ _ Monate weiterarbeiten.

Bei Arbeitgeber 3 möchte ich noch _ _ _ _ _ Monate weiterarbeiten.

5) Wie viel haben Sie brutto pro Stunde verdient?

Bei Arbeitgeber 1 habe ich _ _ _ _ _ Franken pro Stunde verdient.

Bei Arbeitgeber 2 habe ich _ _ _ _ _ Franken pro Stunde verdient.

Bei Arbeitgeber 3 habe ich _ _ _ _ _ Franken pro Stunde verdient.

6) Wie viel (in brutto pro Stunde) halten Sie für angemessen für die Ausübung Ihrer Tätigkeiten?

Für die Ausübung der Tätigkeit bei Arbeitgeber 1 halte ich _ _ _ _ _ Franken pro Stunde für angemessen.

Für die Ausübung der Tätigkeit bei Arbeitgeber 2 halte ich _ _ _ _ _ Franken pro Stunde für angemessen.

Für die Ausübung der Tätigkeit bei Arbeitgeber 3 halte ich _ _ _ _ _ Franken pro Stunde für angemessen.

7) Finden Sie Ihre Teilzeitarbeiten sinnstiftend und erfüllend oder eher frustrierend?

	Sehr frustrierend	Eher frustrierend	Weder noch	Etwas erfüllend	Sehr erfüllend
Arbeitgeber 1	❑	❑	❑	❑	❑
Arbeitgeber 2	❑	❑	❑	❑	❑
Arbeitgeber 3	❑	❑	❑	❑	❑

8) Wie stark fühlen sie sich mit Ihren Arbeitgebern verbunden?

	Sehr schwach	Eher schwach	Weder noch	Eher stark	Sehr stark
Arbeitgeber 1	❑	❑	❑	❑	❑
Arbeitgeber 2	❑	❑	❑	❑	❑
Arbeitgeber 3	❑	❑	❑	❑	❑

9) Fühlten Sie sich in Ihren Teilzeitjobs gefordert?

	Stark unterfordert	Eher unterfordert	Weder noch	Eher gefordert	Stark gefordert
Arbeitgeber 1	❑	❑	❑	❑	❑
Arbeitgeber 2	❑	❑	❑	❑	❑
Arbeitgeber 3	❑	❑	❑	❑	❑

10) Allgemein gesprochen, wie schwierig war es für Sie, eine Teilzeitstelle zu erhalten?

Sehr schwierig	Eher schwierig	Weder noch	Etwas einfach	Sehr einfach
❑	❑	❑	❑	❑

Zuletzt noch einige Fragen zu Ihrer Person.

In welchem Jahr sind Sie geboren? _ _ _ _

Was ist Ihr Geschlecht? ❑ Männlich ❑ Weiblich

Sind Sie in der Schweiz geboren? ❑ Ja ❑ Nein

Falls nicht, wie lange leben Sie schon in der Schweiz?

Ich lebe schon seit _ _ _ _ _ Jahren in der Schweiz.

Wie viele Geschwister haben Sie?

Ich habe _ _ _ _ _ Geschwister.

Über welche abgeschlossenen Schulbildungen verfügen Sie? Bitte geben Sie **alle** abgeschlossenen Schulbildungen an.

- ❑ Primarschule / Volksschule
- ❑ Sekundarschule / Real
- ❑ Lehre / Berufsschule
- ❑ Weiter- / Zweitausbildung
- ❑ Gymnasium
- ❑ Fachhochschule
- ❑ Universität

Falls Ihre Schulbildung noch nicht abgeschlossen ist, geben Sie bitte **zusätzlich** an, welcher Ihr nächster Abschluss sein wird.

- ❑ Primarschule / Volksschule
- ❑ Sekundarschule / Real
- ❑ Lehre / Berufsschule
- ❑ Weiter- / Zweitausbildung
- ❑ Gymnasium
- ❑ Fachhochschule
- ❑ Universität
- ❑ Keine

Herzlichen Dank für Ihre Teilnahme

Appendix to Chapter 2

A Model of Social Comparison

The standard economic model ignores fairness motives and social comparison because it assumes that individuals exclusively pursue their own material interest. Consequently, neither the level of the own flat wage nor the level of coworker wages has an impact on effort in the absence of reputation incentives because material work incentives are identical in each case.

The model of inequity aversion developed by Fehr and Schmidt (1999), however, takes fairness and social comparison into account and provides a micro-foundation of the fair wage-effort hypothesis. Specifically, the level of the flat wage determines firm and worker payoffs and thus influences inequity between the involved parties. An inequity averse worker therefore has an incentive to minimize this inequity by choosing a specific level of effort.

In our setup, a firm employs two identical workers, worker 1 and worker 2. We analyze worker i's effort e_i in response to the wages w_i and w_j set by the firm, and to coworker effort e_j. Let the firm's payoff from worker i be the revenue generated by worker i minus wage cost:

$$\pi_i = ve_i - w_i, i \in \{1, 2\}, \quad e_i, w_i \geq 0.$$

Let worker i's payoff be her wage minus her effort cost:

$$x_i = w_i - ce_i, \quad 0 < c < v,$$

and let worker i's utility be the sum of her payoff x_i, her disutility from inequity with respect to the firm, and her disutility from inequity with respect to her coworker j:

$$\begin{aligned} U_i \;=\; & x_i - \frac{\alpha}{2}\max\{\pi_i - x_i, 0\} - \frac{\beta}{2}\max\{x_i - \pi_i, 0\} \\ & - \frac{\alpha}{2}\max\{x_j - x_i, 0\} - \frac{\beta}{2}\max\{x_i - x_j, 0\} \end{aligned}$$

with the assumption that $\beta \leq \alpha$ and $0 \leq \beta < 1$. The parameter α measures how much the worker dislikes disadvantageous inequity and β measures how much she dislikes advantageous inequity.[21]

[21] When $\alpha, \beta = 0$, the worker only cares about her own payoff, which corresponds to the standard

Effort is not contractible, thus the only reason why a worker should exert effort is to reduce inequity. Consider first inequity between one worker and the firm:

$$|x_i - \pi_i| = |2w_i - (c+v)e_i|.$$

If the worker does not provide effort, i.e. $e_i = 0$, she creates inequity to her advantage of $x_i - \pi_i = 2w_i$. If she works, she will reduce her own payoff but at the same time increase the firm's payoff. One unit of effort decreases inequity with respect to the firm by $c + v$ units. For any wage w_i, the level of effort that equalizes worker and firm payoff is:

$$e_i^{x_i=\pi_i}(w_i) = \frac{2}{c+v} w_i \equiv e^F(w_i).$$

Consider now inequity between worker and coworker. Inequity in relation to the coworker depends on the wages and effort levels of both workers:

$$|x_i - x_j| = |w_i - w_j - c(e_i - e_j)|.$$

Hence, in contrast to worker-firm inequity, one unit of effort changes inequity in relation to the coworker by only c units because own effort does not affect coworker payoff. For wage levels w_i and w_j, and coworker effort e_j, the level of own effort that equalizes worker and coworker payoff is:

$$e_i^{x_i=x_j}(w_i, w_j, e_j) = e_j + \frac{w_i - w_j}{c} \equiv e^C.$$

In particular, worker payoffs are equalized for equal wages if and only if they exert the same amount of effort.

We now analyze a worker's effort choice for different values of α and β. First, if a worker does not suffer enough from advantageous inequity (i.e., for β below a threshold $\underline{\beta}$), she will never exert effort. In other words, marginal utility from inequity reduction with respect to both firm and coworker is lower than marginal cost of effort:

$$\frac{\beta}{2}(c+v) + \frac{\beta}{2} < c, \quad x_i \geq \pi_i.$$

economic model.

This is equivalent to a low value of β:

$$\beta < \frac{2c}{2c+v} \equiv \underline{\beta}.$$

Second, if a worker suffers much from advantageous inequity with respect to the firm, and not too much from disadvantageous inequity with respect to her coworker, she will always exert effort, no matter how much effort her coworker exerts:

$$\frac{\beta}{2}(c+v) - \frac{\alpha}{2}c > c, \quad x_i \geq \pi_i.$$

This corresponds to a low value of α and high value of β:

$$\alpha < \beta\frac{c+c}{v} - 2 \equiv \bar{\alpha}$$

Hence, a worker with these inequity parameters will increase effort as long as her payoff is greater than the firm's payoff. However, no worker ever exerts more effort than the level that equalizes worker and firm payoff, i.e., $e_i = e^F$, even if thereby she could reduce advantageous inequity with respect to her coworker. This is because marginal cost from increased disadvantageous inequity with respect to the firm always outweighs marginal gain from inequity reduction with respect to the coworker:

$$-\frac{\alpha}{2}(c+v) + \frac{\beta}{2}c < 0.$$

As a result, a worker with a low α and high β will always equalize payoffs with the firm by exerting e^F.

Third, if a worker suffers much from both advantageous inequity with respect to the firm and disadvantageous inequity with respect to her coworker, she would like to decrease inequity with respect to the firm because $\beta > \underline{\beta}$, but she is not willing to incur a lower payoff than her coworker because $\alpha > \bar{\alpha}$. Thus, if both α and β are high, she will always equalize payoffs with her coworker rather than with the firm by choosing effort level e^C.

Now we can characterize the three sets of Nash equilibrium strategies for worker 1 and

worker 2 as a function of α and β:

$$\begin{array}{lll} \text{(i)} & \beta < \underline{\beta}: & e_1^* = e_2^* = 0 \\ \text{(ii)} & \beta \geq \underline{\beta}, \alpha < \bar{\alpha}: & e_1^* = e^F(w_i), e_2^* = e^F(w_j) \\ \text{(iii)} & \beta \geq \underline{\beta}, \alpha \geq \bar{\alpha}: & e_1^* = e_1, e_2^* = e_1 + \dfrac{w_2 - w_1}{c}, \quad e_1, e_2 \in [0, e_F] \end{array}$$

Now suppose that the wage can take on two levels, H and L, with $H - L = \Delta > 0$. Consider the situation where both workers earn the high wage H. In case (i), workers provide zero effort in equilibrium. In case (ii), they provide positive effort $e^F(H) = \frac{2}{c+v}H$. In case (iii), any effort level between 0 and $e^F(H)$ that both workers choose is a Nash equilibrium. However, if we assume that workers are able to coordinate on the coalition-proof Nash equilibrium, only two equilibria remain. These two equilibria again depend on the value of β. If β is below a threshold $\bar{\beta}$, workers care little about advantageous inequity with respect to the firm and coordinate on a effort level of 0. Conversely, if β is greater than $\bar{\beta}$, workers care much about inequity with respect to the firm and coordinate on $e^F(H)$:

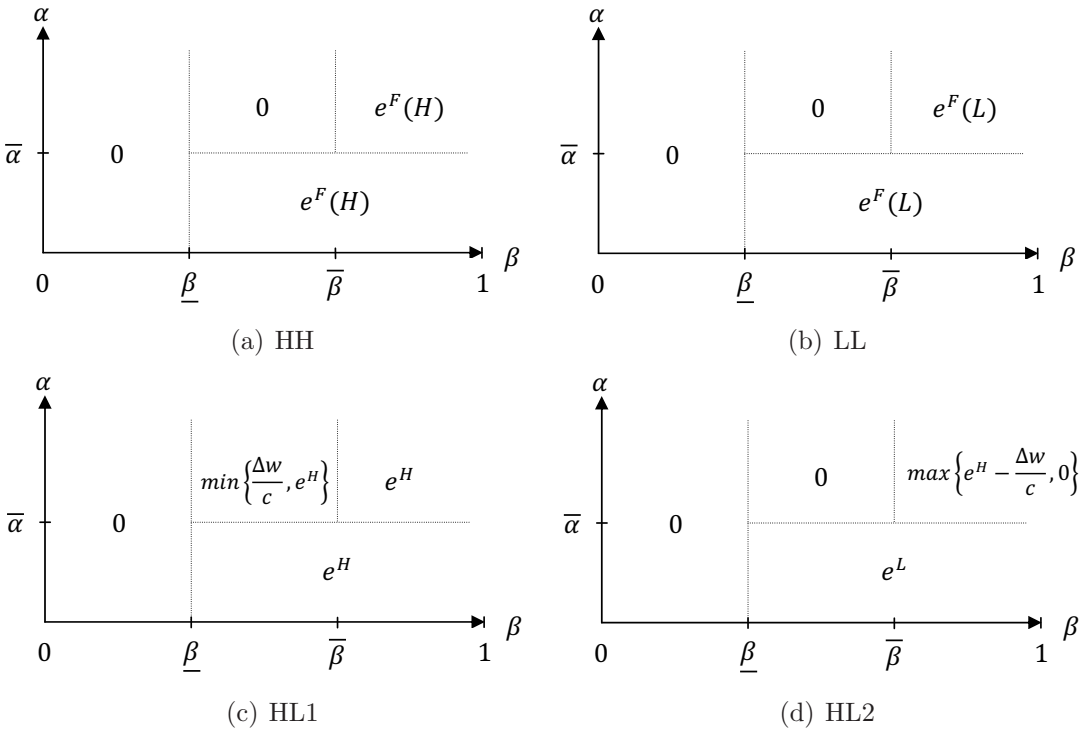
$$\begin{array}{lll} \text{(iii.a)} & \alpha \geq \bar{\alpha}, \underline{\beta} \leq \beta < \dfrac{2c}{c+v} \equiv \bar{\beta}: & e_1^* = e_2^* = 0 \\ \text{(iii.b)} & \alpha \geq \bar{\alpha}, \beta \geq \bar{\beta}: & e_1^* = e_2^* = e^F(H) \end{array}$$

Panel (a) of Figure A shows the equilibrium effort levels if both workers earn the high wage.

The situation where both workers earn the low wage L is analogous to the situation above (see Panel (b) of Figure A). Thus, depending on the values of the inequity parameters, workers choose either $e_1^* = e_2^* = 0$ or $e_1^* = e_2^* = e^F(L)$. In the latter case, a lower wage implies a lower effort level, i.e., $e^F(L) = e^F(H) - \frac{2\Delta}{c+v} < e^F(H)$ because the firm-equalizing effort level $e^F = \frac{2}{c+v}w$ w is proportional to the wage.

Now consider the situation where worker 1 earns the high wage H and worker 2 the low wage L (see Panel (c) and (d) of Figure A). In case (i), where workers do not suffer much from inequity, equilibrium effort is 0 for both workers. In case (ii), where workers equalize their respective payoffs with the firm, equilibrium effort is $e^F(H)$ for worker 1, and $e^F(L)$ for worker 2. In case (iii), workers equalize their payoffs with each other. In case (iii.a), worker 1 has to choose a positive effort because worker 2 cannot provide negative effort. Worker 1 therefore chooses effort $\tilde{e}_1 = \frac{\Delta}{c}$ unless this value exceeds $e^F(H)$, in which case

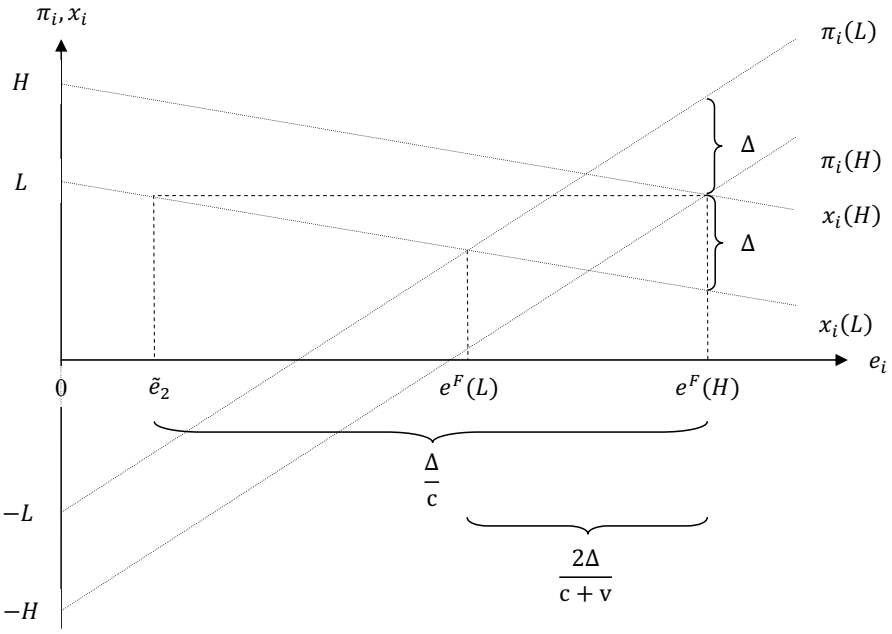
Figure A: Equilibrium effort levels for the four treatment groups



she chooses $e^F(H)$. In case (iii.b), worker 1 chooses $e^F(H)$. She is not willing to provide more effort than $e^F(H)$ because the utility gain from inequity reduction with respect to the coworker would always be lower than the associated effort cost. Thus, in order to equalize worker payoffs, worker 2 chooses $\tilde{e}_2 = e^F(H) - \frac{\Delta}{c}$ unless this value is negative, in which case she chooses 0.

Effort $\tilde{e}_2$ is lower than $e^F(L)$ because worker 2 can influence the payoff of the firm but not her coworker's payoff. As can be seen in Figure B, worker 2 has to reduce effort from $e^F(H)$ to $e^F(L)$ to eliminate 2Δ units of inequity with respect to the firm. The corresponding difference in effort is $\frac{2\Delta}{c+v}$ units because reducing effort by one unit not only increases own payoff by c units but also reduces firm payoff by v units. In contrast, to eliminate Δ units of inequity with respect to the coworker, worker 2 has to reduce effort by $\frac{\Delta}{c}$ units because reducing effort by one unit increases own payoff by c but leaves coworker payoff unaffected. Because marginal product of effort v is greater than marginal cost of effort c, $\tilde{e}_2$ is lower than $e^F(L)$.

Figure B: Payoffs as Functions of Effort



We now derive the hypotheses for the change in effort from pre- to post-intervention period across treatments:[22]

Hypothesis 1 (Treatment LL)

1a *If $\beta < \underline{\beta}$ or $\beta < \bar{\beta}, \alpha \geq \bar{\alpha}$: both workers' equilibrium effort levels remain constant.*

1b *Otherwise: both workers' equilibrium effort levels decrease.*

Hypothesis 2 (Treatment Group HL2)

2a *If $\beta < \underline{\beta}$ or $\beta < \bar{\beta}, \alpha \geq \bar{\alpha}$: worker 2's equilibrium effort level remains constant.*

2b *Otherwise: worker 2's equilibrium effort level decreases.*

2b' *If $\beta \geq \underline{\beta}, \alpha < \bar{\alpha}$: worker 2's equilibrium effort level is the same as in LL.*

2b" *If $\beta \geq \underline{\beta}, \alpha \geq \bar{\alpha}$: worker 2's equilibrium effort level is lower as in LL.*

Hypothesis 3 (Treatment Group HL1)

3a *If $\alpha < \bar{\alpha}$ or $\beta < \underline{\beta}$ or $\beta \geq \bar{\beta}, \alpha \geq \bar{\alpha}$: worker 1's equilibrium effort level remains constant.*

3b *Otherwise: worker 1's equilibrium effort level increases.*

[22]Treatment HH equilibrium effort levels are the same as pre-intervention period equilibrium effort levels because both workers continue to earn the high wage.

Procedural Instructions for Workers and Team Leaders[23]

Nachtausgabe GmbH

Handbuch für
Teamleiter (TL) / Promoter

Stand: 01. Dezember 2008

[23]I merged the instructions for the workers and team leaders and denoted sections that were accessible to only one party accordingly.

1. Das Unternehmen

Das Unternehmen Nachtausgabe GmbH wurde im Oktober 1999 gegründet. Das Ziel des Unternehmens ist es, Endverbrauchern sehr attraktive Ersparnisse in ihrer Freizeit zu ermöglichen und zugleich den teilnehmenden Partnerunternehmen einen Zugewinn durch unsere Tätigkeit zu ermöglichen.
Nachtausgabe.de ist ein Netzwerk über das Angebote zu Veranstaltungen in ganz Deutschland offeriert werden. Darüber hinaus bietet es alles was eine gute Community-Seite auszeichnet.

2. Aktion „Nachtluft schnuppern“

Die Aktion „Nachtluft schnuppern“ findet parallel in Hamburg und Berlin in der Zeit

vom **16. Januar** bis **24. Januar 2009**

statt.

Ziel dieser Aktion ist es Menschen zwischen 18 und 30 Jahren auf unsere Internetpräsenz www.nachtausgabe.de aufmerksam zu machen und neue User zu gewinnen. Um es interessant zu gestalten gibt es diese Aktion.

Nachtluft kombiniert Clubangebote der Städte mit Angeboten von Freizeit- / Lifestyle-Partnern der Region.

Um diese Angebote wahrnehmen zu können erhalten Interessierte (= Kunden) über Promotion-Teams und über Online Bestellung 1 Ticket, welches für ALLE Veranstaltungen gilt – Das **Nachtluft-Ticket**.

Durch Vorzeigen bei unseren Partnern erhält der Kunde die ausgewiesenen Vergünstigungen. Für die Aktion „Nachtluft schnuppern“ findet auch ein Gewinnspiel statt. Dafür registrieren sich Kunden durch ausfüllen des Flyers auf Promotion und erhalten zugleich 2 Tickets kostenlos.
Für den regulären Preis von 5,-€ bekommt der Kunde auch 2 Tickets.

3. Angebote von Nachtluft

ClubPartnerangebote

Angebote der teilnehmenden Clubs und Diskotheken sind verschieden.

Alle Angebote befinden sich auf unserer Homepage: www.nachtausgabe.de/nachtluft. Die Internetseite ist für alle kostenlos. Eine beispielhafte Zusammenstellung ist der Flyer. Er beinhaltet aus Platzgründen nicht alle Angebote.

Beispiele für Clubangebote: kostenloser Eintritt oder Freigetränke.

LifestylePartner-Angebote

In dieser Rubrik befinden sich hauptsächlich Dienstleistungen rund ums Thema Freizeit, Spaß und Spiel. Alle Angebote mit Beschreibung der Anbieter befinden sich auf unserer Homepage: www.nachtausgabe.de/nachtluft.

Beispiele für Lifestyle-Angebote: Rabatte, kostenlose Proben, 2für1-Angebote

Voting/Competition

Jeder Kunde kann sich auf unserer Homepage anmelden und Bilder online stellen. Mit diesen Bildern kann er an einem Voting teilnehmen, welches jeder Online-Besucher von Nachtausgabe.de beeinflussen kann. Hierbei wird „Das Gesicht von Nachtluft" gesucht. Es ist sehr kreativ angelegt, so dass jeder Kunde darauf hingewiesen werden sollte. Nach Möglichkeit werden auf unseren Promotion erste Bilder von Kunden gemacht und online gestellt. Auf den Gewinner der Competition warten tolle Gewinne.

Gewinnspiel

Am Gewinnspiel nehmen alle Personen teil, die über Promotion den Flyer vollständig ausgefüllt haben. Die Gewinnspielauswertung findet nach Abschluss der Veranstaltung statt. Gewinner werden angeschrieben und auf www.nachtausgabe.de veröffentlicht. Alle Gewinne sind auf unserer Internetpräsenz angegeben. Gewinne sind z.B. ein Partygutschein und Sachpreise.

4. Promotionablauf

Street- und Clubpromotion dauern jeweils 3 Stunden. Es gibt keine Verlängerung oder vorzeitigen Abbruch. Die Streetpromotion findet immer in den Zeiten zwischen 17.00 Uhr - 20.00 Uhr an Hotspots der Stadt statt und die Clubpromotion zwischen 23.00 Uhr - 02.00 Uhr in Clubs und Diskotheken. (In Hamburg nach Absprache)
Zeitgleich sind Teams im Aktionsraum Berlin und Hamburg im Einsatz.
Jeder Teamleiter (TL) betreut zwei Teams, jedes Team besteht aus zwei Promotern. Der TL ist für Fragen aller Art der Ansprechpartner.

Vorbereitung (TL)

Einige Tage vor Start der ersten Promotion erhalten **Teamleiter** die Einsatzpläne, Materialien und Kleidung für alle Promotionen im Dezember-Block. Das beinhaltet: Flyer, Tickets, Datenblätter zur Abrechnung, Klemmbretter, Brustbeutel, Hüfttaschen, Schlüsselbänder, TL-Tasche, Kugelschreiber, T-Shirts und Windbreaker.

Ausstattung/Stück (TL):

Jeder **Street-Promoter** erhält:

- 1x Windbreaker, gelb
- 1x Klemmbrett
- 1x Schlüsselband, nachtausgabe
- 1x Brustbeutel, schwarz
- 1x Hüfttasche, schwarz
- 2x Kugelschreiber
- 100 Tickets
- 120 Flyer
- 5 Datenblätter–Adresse
- 1x CashListe

Jeder **Club-Promoter** erhält:

- 1x T-Shirt, gelb
- 1x Klemmbrett
- 1x Schlüsselband, nachtausgabe
- 1x Brustbeutel, schwarz
- 1x Hüfttasche, schwarz
- 2x Kugelschreiber
- 100 Tickets
- 100 Flyer
- 5 Datenblätter–Adresse
- 1x CashListe

Reserven und TL-Ausstattung:

- 1x TL-Tasche, schwarz
- 2x Jacken (für jedes Team stehen die gleichen 3 Größen zur Verfügung, die jeweils nicht genutzte verbleibt)
- 1x Klemmbrett
- 2x Schlüsselband
- 2x Brustbeutel
- 1x Hüfttasche
- 5x Kugelschreiber
- restl. Tickets (von Ausgang 3000)
- restl. Flyer (von Ausgang 3000)
- Datenblätter
    1. Streetpromotionbereiche
    2. Adressenblätter
    3. Promoterprotokoll
    4. Teamleiterprotokoll

Anmeldung: TLs werden die generierten Daten der Promotion in unsere OnlineDatenbank eintragen. Dazu ist erforderlich, dass sich jeder zuvor auf www.nachtausgabe.de einen Account anlegt und seinen User-Namen dem NA-Büro Berlin mitteilt. Damit kann dann die Freischaltung erfolgen.

Die Einteilung in Verdienstgruppen (TL)

Die Universität Zürich führt im Rahmen der Nachtluft-Aktion eine wissenschaftliche Studie durch.

Jedes Promoterteam besteht aus einem „1"-Promoter und einem „2"-Promoter. Diese Aufteilung hat nichts mit der Tätigkeit der Promoter zu tun, diese ist für beide Promoter identisch! Jedoch hat die Aufteilung in „1"- und „2"-Promoter einen Einfluss auf den Verdienst der Promoter.

In der ersten Woche (d.h. FR / SA in Woche 1) verdienen ALLE Promoter (d.h. sowohl „1"-Promoter als auch „2"-Promoter) **12 €** anstelle der vertraglich vereinbarten 10 € pro Stunde.

In der zweiten Woche (d.h. FR / SA in Woche 2) werden die Promoterteams in **3** unterschiedliche Gruppen eingeteilt.

HH-Gruppe: Sowohl „1"-Promoter als auch „2"-Promoter erhalten WEITERHIN **12 €** pro Stunde (HH = „1"-Hoch & „2"-Hoch).

TT-Gruppe: Sowohl „1"-Promoter als auch „2"-Promoter erhalten NEU **9 €** pro Stunde (TT = „1"-Tief & „2"-Tief).

H_T-Gruppe: NUR „2"-Promoter erhält NEU **9 €** pro Stunde, „1"-Promoter erhält WEITERHIN **12 €** pro Stunde (H_T = „1"-Hoch & „2"-Tief).

HH	Woche **1** (FR / SA)		Woche **2** (FR / SA)
„**1**"-Promoter:	**12** €/Stunde	→	**12** €/Stunde
„**2**"-Promoter:	**12** €/Stunde	→	**12** €/Stunde

TT	Woche **1** (FR / SA)		Woche **2** (FR / SA)
„**1**"-Promoter:	**12** €/Stunde	→	**9** €/Stunde
„**2**"-Promoter:	**12** €/Stunde	→	**9** €/Stunde

H_T	Woche **1** (FR / SA)		Woche **2** (FR / SA)
„**1**"-Promoter:	**12** €/Stunde	→	**12** €/Stunde
„**2**"-Promoter:	**12** €/Stunde	→	**9** €/Stunde

In allen Teamleiter-Unterlagen werden die Gruppen jeweils mit **HH**, **TT** und **H_T** bezeichnet. Die Gruppenzugehörigkeit ist im Einsatzplan vermerkt.

Das Treffen vor einem Promotionseinsatz (TL)

Den Treffpunkt für die Teams vereinbart der TL. Die Treffen mit den unterschiedlichen Teams finden im Abstand von 15 Minuten statt (d.h. die Teams begegnen sich gegenseitig nicht!).

VOR JEDEM TREFFEN: Der Teamleiter bereitet alle Materialien vor, die er für und die Promoter für die Promotion benötigen. Jacken und andere Promotionsmaterialien (s.o.) werden vor jeder Promotion verteilt und danach wieder eingesammelt. Der TL ist dafür verantwortlich, dass alle Promotionsunterlagen vorbereitet sind, insbesondere werden die Teamleiter- und Promoter-Protokolle korrekt **vorausgefüllt** (mit Datum, Aktionsort, Promoternamen, Verdienstgruppe (HH, TT oder H_T)). Die Informationen dafür entnimmt der TL dem Einsatzplan.

Für die **ClubPromotion** gibt der TL gibt am ersten Tag des 2-Wochen-Blocks T-Shirts aus, die er erst am letzten Tag wieder einsammelt.

Damit pünktlich mit der Promotion begonnen werden kann, vereinbart der TL ein Treffen für die Übergabe 20-30min vorher. Eventuelle Besonderheiten für die folgende Promotion werden hier besprochen.

Bekanntgabe von Lohnänderungen (TL)

Woche **1**:

ALLE Gruppen, Lohn**erhöhung**

„Alle Promoter erhalten **12 €** statt 10 € pro Stunde. Das hat der Chef so bestimmt."

Woche **2**:

Gruppe **HH**, **gleicher** Lohn

„Alle Promoter erhalten weiterhin **12 €** pro Stunde. Das hat der Chef so bestimmt."

Gruppe **TT**, Lohn**senkung** für beide

„Alle Promoter erhalten ab jetzt **9 €** statt 12 € pro Stunde. Das hat der Chef so bestimmt."

Gruppe **H_T**, Lohn**senkung** für „2"-Promoter

„Alle „2"-Promoter erhalten ab jetzt **9 €** statt 12 € pro Stunde. Das hat der Chef so bestimmt."

WICHTIG: Für die Kommunikation mit den Promotern (unter anderem bei Nachfragen) gelten besondere Regeln (s.u. „Kommunikationsrichtlinien").

Durchführung eines Promotionseinsatzes (TL)

Der Teamleiter

1. ist der erste am Treffpunkt und bereitet Übergabe vor
2. gibt den Aktionsradius (Streetpromotion) vor
3. achtet darauf, dass die Promoter möglichst weit voneinander entfernt aufgestellt sind
4. macht nach Möglichkeit Fotos von „Kunden“
5. ist in regelmäßigem Abstand bei beiden Teams vor Ort
6. versorgt mit Nachschub
7. entsorgt vom Kunden weggeworfenes Material (Flyer,Tickets)
8. achtet auf die zeitliche Begrenzung

Ende eines Promotionseinsatzes (TL)

Der TL sammelt die Materialien und Unterlagen wieder ein. Er rechnet zusammen mit den Promotern die verteilten / verkauften Ticketsätze ab und füllt mit ihnen das Promoterprotokoll aus. Dann können die Promoter gehen.

DANACH füllt der TL das Teamleiterprotokoll aus.

Nacharbeit (TL)

Der TL trägt Datensätze in die OnlineDatenbank von nachtausgabe.de ein. Dazu gibt es eine kurze Einweisung und kann dann nach jeder Promotion erfolgen. Die ausgefüllten Promoterprotokolle, Teamleiterprotokolle, Flyer-Abrisse, Cash-Listen usw. sind nach Ende der letzten Promotion im Dezember im NA-Büro Berlin abzugeben. Bargeld kann gegen Quittung auch vorher schon bei den Verantwortlichen Rafael Armbrust oder Oliver Schabelski abgegeben werden. Den gleichen Ablauf gibt es dann wieder im Januar vor dem Start unserer Aktionswoche.

5. Überblick Promoter Tätigkeit

Aufgaben

Bei kurzen und prägnanten Ansprachen ist mit dem Ausfüllen des Flyers ein Gespräch nach 3min beendet. Wir kalkulieren jedoch mit einem Schnitt von 4-5min. Das ergibt 10-20 Ticketsätze je Stunde.

Unsere Promotion konzentriert sich auf 3 Stunden je Einsatz. Wir sind an den Hotspots und in den angesagtesten Clubs der Stadt unterwegs. Wir finden mehr potentielle Gesprächspartner vor als wir ansprechen können.

Die Aufgabe eines jeden Promotionsmitarbeiter ist es jede Person im geschätzten Alter von 18-30 Jahren anzusprechen und möglichst kurz und gezielt auf die Aktion hinzuweisen, mit dem Ergebnis:
Je 2 Tickets

- für 5,00€ verkauft oder
- Anmeldung des Kunden durch Ausfüllen des Flyer

Bei Ausfüllen des Flyers wird die Anmeldung abgetrennt und verbleibt bei uns! Der Kunde erhält den Flyer.
Die Karten werden sorgsam bis zum Promotionsende aufbewahrt und dann dem Teamleiter (TL) übergeben. Anschließend wird auch das eingenommene Geld mit dem TL verrechnet.

6. Kleidung- und Materialhandhabung

STREET-Promotion – Ausstattung

Hinweis: Bitte warm anziehen!

Vorn

Erkennung:
1. Windbreaker, gelb (AktionsBrand)
2. Schlüsselband
3. Klemmbrett zur Präsentation

Materialvorrat:
4. Gürteltasche, schwarz
(unter der Jacke zu tragen)
5. Brustbeutel, dunkel-blau
(unter der Jacke zu tragen)

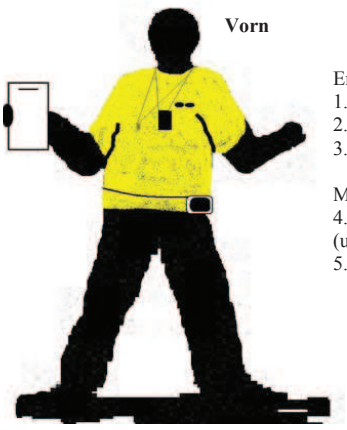
Hinten

Gürteltasche
In der Gürteltasche werden Flyer und Tickets aufbewahrt
Brustbeutel
In den Brustbeutel werden ausgefüllte Gewinnspielkarten (Flyer-Abriss) eingesteckt, sowie entgegengenommenes Bargeld.
Klemmbrett
Präsentation Flyer,
Listen – Notieren der Daten des Teilnehmers durch Promoter (nur bei trockenem Wetter)

CLUB-Promotion – Ausstattung

Hinweis: Bitte für Club – schwarze Hose + schwarzer Pulli!

Vorn



Erkennung:
1. T-Shirt, gelb (AktionsBrand)
2. Schlüsselband
3. Klemmbrett zur Präsentation

Materialvorrat:
4. Gürteltasche, schwarz
(unter dem Shirt zu tragen)
5. Brustbeutel, dunkel-blau

Hinten

7. Do's and Don´ts (Promoter)

Do's

- ✓ Einträge in den Promotionsplan sind verbindlich
- ✓ Pünktlich beim verabredeten Treffpunkt erscheinen
- ✓ Bei eigenem Ausfall mind. 6h vorher bescheid geben
- ✓ Begeisterung für die Veranstaltung und Spaß an der Tätigkeit
- ✓ Gut gelaunt und überzeugend sein
- ✓ Zügige Ansprachen
- ✓ Lesbarkeit und Richtigkeit der Angaben garantieren, dazu selbst für den Kunden den Flyer ausfüllen und den Ausweis/Personalien zeigen lassen (kostenlose Tickets gibt's nur für richtige Angaben, sonst 5€)
- ✓ Sorgfältiger Umgang mit überlassenen Materialien
- ✓ Auf jeder Anmeldung Kürzel notieren
- ✓ Rückfahrt selbstständig planen. Die Teamleiter sind dafür nicht zuständig.

Don'ts

- ✓ Kurzfristiges Absagen
- ✓ Unpünktlichkeit
- ✓ Schlecht gelaunt sein und im Team seine schlechte Laune verbreiten
- ✓ alkoholische Getränke während der Promotion trinken
- ✓ Lange Raucherpausen (max. 5 Minuten pro Stunde)

Ansprechpartner

Für die Tätigkeit vor Ort ist Ihr Ansprechpartner immer Ihr zugewiesener Teamleiter, der für Sie auch alle Informationen zu Ihrer Tätigkeit bereit hält. Für Rückfragen steht Ihnen die nachfolgende Adresse des Nachtausgabe Regionalbüros zur Verfügung.

8. Kommunikationsrichtlinien (TL)

In Gegenwart von Promotern AUF KEINEN FALL erwähnen:

- ✓ Studie der Universität Zürich
- ✓ Andere Verdienstgruppen
- ✓ Den anderen 2-Wochen-Block
- ✓ Nachträgliche Kontrolle der gesammelten Adressen

In Gegenwart von Promotern betonen:

- ✓ Die Nachtluft-Aktion ist einmalig, keine spätere Weiterbeschäftigung
- ✓ Einteilung in „1"- und „2"-Promoter ist zufällig geschehen und hat nichts mit der Promotertätigkeit zu tun. Einteilung hat abrechnungstechnische Gründe
- ✓ Das Promotionsziel von 10-20 Ticketsätzen pro Stunde ist unverbindlich und hat keine Auswirkung auf den Verdienst

Bei Nachfragen von Promotern antworten:

- ✓ „Warum gibt es Promoter 1 und 2?"
  Antwort: *Das wurde nur aus abrechnungstechnischen Gründen eingeführt.*
- ✓ „Warum ändert sich der Lohn?"
  Antwort: *Keine Ahnung, das hat der Chef so entschieden.*
- ✓ „Ich habe erfahren, dass andere Teams anders bezahlt werden…"
  Antwort: *Davon ist mir nichts bekannt, ich werde das abklären.*
- ✓ Alle anderen Fragen/Kommentare:
  Antwort: *Das ist halt so, der Chef hat uns das so mitgeteilt.*

9. Verhaltensrichtlinien (TL)

- ✓ Alle Promoter GRUNDSÄTZLICH GLEICH behandeln, d.h. insbesondere keine unterschiedlichen Motivationen oder Bestrafungen aussprechen.
- ✓ Keine aktive Leistungskontrolle der Promoter. Die Promoter dürfen sich nicht ständig beobachtet oder kontrolliert fühlen (z.B., wenn der TL zum Fotografieren vorbeikommt).

10. Zahlen zum Nachtluft Ticket

✓ Ticketsatz (=2stk), regulär	5,-€
✓ Ticketsatz (=2stk) bei Anmeldung	kostenlos (1x pro Kunde)
✓ Gültigkeit	9 Tage (16.1.-24.1.09)
✓ In Berlin & Hamburg gültig bei über 50 Partnern	

Bitte setzen Sie die Anleitungen in Ihrem Handbuch um, damit alle Ziele erreicht werden und auch alles ordnungsgemäß abgerechnet werden kann.

Herzlichen Dank – Ihr Nachtausgabe-Team

Accounting Protocol for Team Leaders

Hotspot Promotion **17.00 – 20.00 Uhr**

Aktionsort _____ _____

Datum _____ **(TT.MM.JJJJ)**

	Name	**Vorname**	**# Kartensätze ↔ Adresse**	**# Kartensätze ↔ €5**	**Beginn (hh.mm)**	**Ende (hh.mm)**	**Lohn (/Std.)**	**Lohn (gesamt)**	**Unterschrift Promoter**
Promoter 1									
Promoter 2									

Anzahl Passanten

Sehr wenige **Wenige** **Mittel** **Viele** **Sehr viele**

Parallel-Promotion/ Konkurrenz vor Ort

☐ **Ja** **Wenn Ja, welche?** _____

☐ **Nein**

Bekannte angetroffen

Promoter 1	☐ **Keine**	☐ **Wenige**	☐ **Viele**
Promoter 2	☐ **Keine**	☐ **Wenige**	☐ **Viele**

Appendix to Chapter 3

Procedural Protocol for Research Assistants

Checkliste

GENERELLES

Aufgabenaufteilung
- Hiwi1 leitet die Gespräche
- Hiwi2 kommt erst bei der Raumeinteilung zum aktiven Einsatz

Erwähne auf keinen Fall, …
- dass ein Experiment stattfindet!
- wie viele Bucheinträge erwartet werden!

Es ist wichtig, …
- dass ähnliche Fragen auf gleiche Weise beantwortet werden!
- dass keine willkürlichen Begründungen abgegeben werden!

1. VOR DEM ERSCHEINEN DER STUDENTEN

Arbeitsplätze Studenten:
- □ Räume mit „Büchereingabe Staatswissenschaftliche Bibliothek – Bitte nicht stören" kennzeichnen
- □ Räume öffnen, Licht anmachen
- □ Computer anmachen
  - □ Eingabemaske starten (CSV-Datei wird dann automatisch auf dem Desktop generiert)
  - □ Internet Explorer-Link sichtbar
  - □ Keine weiteren Dateien auf dem Desktop
- □ 250 Bücher und Kisten für erledigte Bücher bereitlegen.

Aufsichtsplatz Hiwi1:
- □ Gruppen- und Raumeinteilung (**GEMISCHTER/GLEICHER RAUM**), sowie Lohnsystem (**TEAM/WETTBEWERB**) überprüfen
- □ Anleitungen für die Büchereingabe bereitstellen
- □ Merkblätter Entlohnung bereitstellen (**TEAM** blaues Blatt, **WETTBEWERB** grünes Blatt)
- □ Rechnungen bereitstellen
- □ Protokoll versteckt (!) bereitstellen

Arbeitsplatz Hiwi2:
- □ Codierblätter bereitstellen

2. STUDENTEN EMPFANGEN

Vor der Eingangstür:
- □ Sind alle Studenten vor der Eingangstür, dann begrüße sie: *Sind sie die Aushilfen für die Büchereingabe? Herzlich Willkommen zum Job für die Büchereingabe, mein Name ist [Name], bitte folgen sie mir.*
- □ FEHLT EIN STUDENT: Rufe ihn auf sein Handy an und versuche die Arbeitsgruppen zu vervollständigen
- □ Führe die Studenten zu einem Arbeitsplatz
  - □ Vorführ-Laptop mitnehmen
  - □ Anleitungen für die Büchereingabe mitnehmen
  - □ Merkblätter Entlohnung mitnehmen
  - □ Zeige ihnen den Weg zur nächsten Toilette (auf dem Weg zum Arbeitsplatz erledigen.)

3. BÜCHEREINGABE ERKLÄREN

Hintergrund der Büchereingabe erklären:

- □ *Bibliotheksbestände der Staatswissenschaftliche Bibliothek sollen elektronisch erfasst werden. Bisher wurden die Bücherbestände noch nicht einheitlich für ein elektronisches Datenbanksystem erfaßt. Damit dies in Zukunft einfacher wird, brauchen wir diese Daten elektronisch erfasst.*
- □ *Ihre Anstellung ist auf 4 Stunden begrenzt* (BEI RÜCKFRAGEN: *es war unklar, wie viele Studenten sich melden würden, ebenso wie lang die gesamte Erfassung dauern würde. Dieses Eintragen in kleinen Schritten erleichtert die Organisation*.).

Büchereingabe erklären:

- □ Anleitungen für die Büchereingabe überreichen: *Bitte lesen sie als Erstes die Anleitung für die Büchereingabe sorgfältig durch. Die Anleitung erläutert jeden Arbeitsschritt und soll sie bei der Büchereingabe unterstützen.*
- □ Vorführung Büchereingabe am Laptop (nicht an einem Computer der Aushilfen)
  - □ Eingabemaske starten und erklären
  - □ Bücherinformationen beispielhaft zeigen
  - □ Bucheingabe vormachen
  - □ Nochmals auf „Besondere Hinweise“ und „Problemlösung“ eingehen
- □ Jeder Student erfasst ein Buch – Kontrolle ob Eintrag korrekt
- □ *Haben sie noch irgendwelche Fragen betreffend der Büchereingabe?*

Arbeitsgruppen (A und B) bilden:

- □ *Bevor wir sie nun zu ihrem Arbeitsplatz führen, teile ich sie in Arbeitsgruppen mit jeweils 2 Personen ein. Ihr Lohn ist abhängig von der Leistung der anderen Person in ihrer Arbeitsgruppe. Wie sich ihr Lohn genau bestimmt erklären wir Ihnen später wenn sie sich an ihrem Arbeitsplatz eingerichtet haben.*
  - □ BEI RÜCKFRAGEN: *das machen wir immer so und das hat sich so bewährt.*

Raumeinteilung:

- □ Die Studenten werden gemäß ihrer Raumeinteilung zu ihren Arbeitsplätzen geführt – **jeder Hiwi ist für einen Raum verantwortlich!**
- □ **GEMISCHTER RAUM**: Studenten *A1 und B1 gehen bitte mit [Hiwi2] mit, A2 und B2 bleiben bei mir.*
- □ **GLEICHER RAUM**: Studenten *A1 und A2 gehen bitte mit [Hiwi2] mit, B1 und B2 bleiben bei mir.*
- □ BEI RÜCKFRAGEN: *Davon habe ich nicht so viel Ahnung, aber es wurde mir gesagt, dass ich darauf achten soll, und deshalb mache ich das jetzt so.*
- □ FEHLT EIN STUDENT: **GLEICHER RAUM** und Student aus unvollständiger Arbeitsgruppe sitzt alleine im Raum (falls zwei Studenten fehlen, bilde eine Arbeitsgruppe und **GLEICHER RAUM**)

Entlohnung:

- □ **TEAM**: *Ihr Lohn besteht aus (i) einem festen Stundenlohn und (ii) einem leistungsabhängigen Lohn. Der feste Stundenlohn beträgt €3.50/h. Der leistungsabhängige Lohnanteil ist abhängig von der Anzahl **vollständiger** Bucheinträge ihrer Arbeitsgruppe. Am Schluss werden die Bucheinträge von ihnen und ihrem Gruppenmitglied **zusammengezählt**. Jeder Bucheintrag dieser Summe wird mit €0.20 vergütet. Der Gesamtbetrag wird anschliessend gleichmässig auf beide Gruppenmitglieder aufgeteilt. Die Berechnung ihres Lohnes ist nochmals auf dem Merkblatt erklärt.* Merkblatt für die Entlohnung (blau) übergeben.
  - □ FALLS STUDENT ALLEINE ARBEITET: *Lohn besteht aus (i) einem festen Stundenlohn und (ii) einem leistungsabhängigen Lohn. Der feste Stundenlohn beträgt €3.50/h. Der leistungsabhängige Lohnanteil ist abhängig davon wie viele Bücher Sie erfasst haben. Pro Bucheintrag erhalten Sie zusätzlich €0.10.* KEIN MERKBLATT FÜR DIE ENTLOHNUNG ÜBERGEBEN.

□ **WETTBEWERB**: *Ihr Lohn besteht aus (i) einem festen Stundenlohn und (ii) einem leistungsabhängigen Lohn. Der feste Stundenlohn beträgt €11.00/h. Der leistungsabhängige Lohnanteil ist abhängig von der Anzahl **vollständiger** Bucheinträge die sie im Vergleich zu ihrem Gruppenmitglied eingeben. Für jeden Bucheintrag, den sie **mehr** eingeben als ihr Gruppenmitglied erhöht sich ihr Lohn um €0.10. Für jeden Bucheintrag den sie **weniger** eingeben als ihr Gruppenmitglied verringert sich ihr Lohn um €0.10. Das Gleiche gilt für ihr Gruppenmitglied. Die Berechnung ihres Lohnes ist nochmals auf dem Merkblatt erklärt.* Merkblatt für die Entlohnung (grün) übergeben.

□ FALLS STUDENT ALLEINE ARBEITET: *Ihr Lohn beträgt €11.00/h.* KEIN MERKBLATT FÜR DIE ENTLOHNUNG ÜBERGEBEN.

□ *Da ihre Arbeitsdauer 4 Stunden beträgt, ist ihr Arbeitseinsatz um [Zeit] Uhr zu Ende. Ich werde dann vorbeikommen und ihnen den Lohn auszahlen.*

□ *Wir bitten sie, die Büchereingabe möglichst sorgfältig zu erledigen. Geben sie so viele Bücher ein wie möglich. Sie haben genügend Bücher vor sich, sodass sie ungestört durcharbeiten können. Falls Ihnen die Bücher ausgehen sollten, sagen Sie mir bitte kurz Bescheid. Ich lasse sie nun alleine, bis später.*

□ Senden von SMS an Hiwi, der im Labor sitzt zur Orientierung über Endzeit der Büchereingabe

4. WÄHREND DER BÜCHEREINGABE

Protokoll führen (am Aufsichtsplatz Hiwi1)

Lohnauszahlung vorbereiten:

□ Geld

□ Rechnungen

□ Falls alleine gearbeitet wurde, muss der Personalfragebogen (siehe Rückseite Rechnung) nicht ausgefüllt werden

5. NACH DER BÜCHEREINGABE

Lohnauszahlung (nach 4 Stunden Arbeitszeit)**:**

□ **Jeder Hiwi ist jetzt für eine Arbeitsgruppe verantwortlich**

□ **GLEICHER RAUM**: jeder Hiwi geht in den Raum für den er verantwortlich ist

□ **GEMISCHTER RAUM**: beide Hiwis gehen zuerst in denselben Raum (sagen vorher den Aushilfen im anderen Raum, dass die Zeit nun zu Ende sei und sie aufhören können), bestimmen die Leistung des Studenten ihrer Arbeitsgruppe – dann wechselt ein Hiwi den Raum zusammen mit seinem Studenten und schickt den Studenten der anderen Arbeitsgruppe aus dem anderen Raum zum anderen Hiwi

□ Löhne bestimmen (Taschenrechner) und Rechnungen (inkl. Personalfragebogen) ausfüllen lassen

□ BEI RÜCKFRAGEN: *Wir würden uns z.B. gerne anschauen, inwiefern der Tippgeschwindigkeitstest verbessert werden kann, damit wir auch in Zukunft gute Aushilfen rekrutieren können.*

□ Beim Ausfüllen der Rechnungen (inkl. Personalfragebogen) möglichst die Kommunikation zwischen den Gruppenmitgliedern verhindern

□ *Hier ist ihr Geld. Besten Dank für Ihren Einsatz.*

6. EINLADUNG VERHALTENSSTUDIE

□ *Haben sie Interesse noch zusätzlich Geld zu verdienen? …*

□ *Forscher der Volkswirtschaftlichen Fakultät führen zurzeit eine größere Studie durch und benötigen noch Studienteilnehmer. Wir wurden angefragt, nach zusätzlichen Studienteilnehmern Ausschau zu halten. Wenn Sie möchten, können sie sofort an der Studie teilnehmen. Ein Hiwi wird gleich vorbeikommen um sie abzuholen. Das Ganze dauert inkl. Weg etwa 30 Minuten. Sie erhalten für die Teilnahme €7.00 sofort in bar ausbezahlt und können zusätzlich noch mehr Geld verdienen.*

□ Manuel steht schon vor der Tür und erläutert noch die Studie etwas genauer.

- □ Falls ja: Hiwi begleitet Studenten zum MELESSA
- □ Falls keine Zeit: *Sie haben auch die Möglichkeit zu einem späteren Zeitpunkt an der Studie teilzunehmen. Schreiben sie Ihre Email-Adresse auf damit wir diese weitergeben können. Sie werden dann von der Studienleitung zur Vereinbarung eines Termins kontaktiert.*

7. AUFRÄUMARBEITEN

Daten speichern:
- □ CSV-Datei auf Memory Stick speichern und bezeichnen (ID Aushilfe gemäß Einsatzplan)
- □ CSV-Dateien Hiwi-Notebook speichern

Codierblätter ausfüllen

Arbeitsplätze aufräumen und für den nächsten Einsatz vorbereiten:
- □ Schmierereien und alte (i) Merkblätter Entlohnung und (ii) Anleitungen für die Büchereingabe von den Arbeitsplätzen entfernen!
- □ Bücher bereitlegen
- □ CSV-Datei auf dem Desktop und im Papierkorb löschen

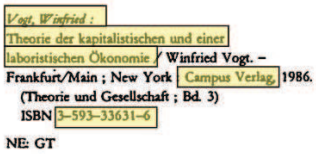
Task Instructions for Workers

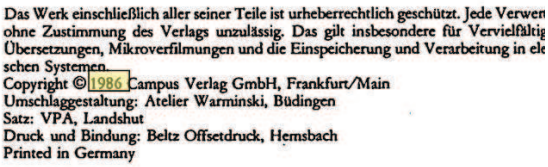
Anleitung für die Büchereingabe

Für jedes Buch müssen Sie folgende Informationen eingeben:

- Vollständiger **Titel** und **Untertitel**
- **Autor** (Bsp.: Vorname Nachname)
- **Weitere Autoren** (falls vorhanden), mit ";" trennen (Bsp.: Vorname Nachname; Vorname Nachname)
- **Verleger/Verlag**
- **ISBN-Nummer,** ohne "-" eingeben (Bsp.: 1234567890)
- **Erscheinungsjahr**

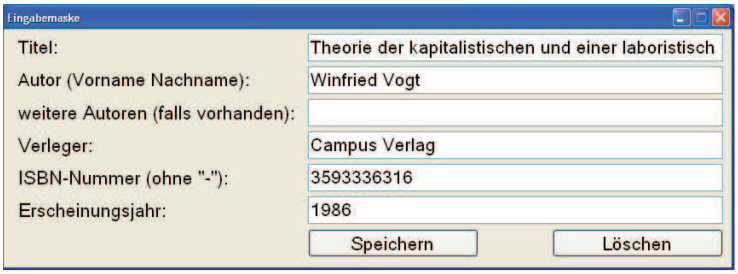
Auf den ersten Seiten eines jeden Buches findet sich immer eine Seite ähnlich der Folgenden:





- **Klicken** Sie mit der linken Maustaste in das erste Feld der Eingabemaske, um mit der Eingabe zu beginnen
- Mit der Taste '**Tab**' oder per Mausklick wechseln Sie von einem Feld zum nächsten
- Nutzen Sie den Nummernblock, um die ISBN-Nummer einzugeben.
- Wenn Sie ein Buch vollständig eingegeben haben, können Sie mit der Taste '**Enter**' oder per Mausklick auf **'Speichern'** das nächste Buch eingeben.
- Die aktuelle Eingabe kann per Mausklick auf **'Löschen'** gelöscht werden.

Die Eingabe für das obige Beispiel sieht wie folgt aus:



Besondere Hinweise:

- Titel wie "Professor", "Dr.", etc. müssen nicht eingegeben werden
- Bei "seltsamen" Buchstaben (wie Ø etc.) nehmen Sie den "ähnlichsten" lateinischen Buchstaben
- Römische Ziffern als Großbuchstaben eingeben (Bsp.: "IV" anstatt "iv")
- Bei Sammelbänden nehmen Sie den Herausgeber als Autor
- Folgende Angaben sollten nicht als Autoren erfasst werden:
  - Reihenherausgeber (Bsp.: "Schriften von …")
  - Übersetzer
- Wenn es mehrere Erscheinungsjahre gibt, dann geben sie das aktuellste ein
- Bei mehreren ISBN-Nummern geben Sie immer die erste ein
- Geben Sie immer die ISBN-Nummer ein, nicht die ISSN-Nummer
- Doppelte Bücher müssen getrennt eingegeben werden
- Klappen Sie nicht das Display vom Laptop herunter. Dadurch wird er in den Ruhezustand versetzt
- Bitte ändern Sie nichts an den Einstellungen des Laptops

Problemlösung:

- Bei Computerabsturz den Computer neu starten und per doppeltem Mausklick die Eingabemaske neu starten
- Falls die Eingabemaske nicht mehr funktioniert, mit den Tasten '**Strg**' + '**Alt**' + '**Entf**' die Anwendung beenden und die Eingabemaske neu starten

Falls Sie ein Problem nicht selbst lösen können, melden Sie sich bei der Hilfskraft draußen im Gang.

Incentive Scheme Handout for Workers

Merkblatt für die Entlohnung

[Wettbewerb] Ihr Einkommen besteht aus:

- einem festen Stundenlohn von **€11.00/Std** und
- einem leistungsabhängigen Lohn. Dieser ist abhängig von der Anzahl **vollständiger Bucheinträge**, die sie im Vergleich zu ihrem Gruppenmitglied eingeben. Für jeden Bucheintrag den sie **mehr** eingeben als ihr Gruppenmitglied erhöht sich ihr Lohn um **€0.10**. Für jeden Bucheintrag den sie **weniger** eingeben als ihr Gruppenmitglied verringert sich ihr Lohn um **€0.10**. Das Gleiche gilt für ihr Gruppenmitglied.

Ihr Einkommen setzt sich wie folgt zusammen:

Fester Stundenlohn

€11.00/Std · 4 Std = €44.00

+/-

Leistungsabhängiger Lohn

In Abhängigkeit ob Sie mehr oder weniger Bücher eingetragen haben:

Lohnaufschlag	Anzahl Bücher, die Sie **mehr** eingetragen haben · €0.10
Lohnabzug	Anzahl Bücher, die Sie **weniger** eingetragen haben · €0.10

[Team] Ihr Einkommen besteht aus:

- einem festen Stundenlohn von **€3.50/Std** und
- einem leistungsabhängigen Lohn. Dieser ist abhängig von der Anzahl **vollständiger Bucheinträge** ihrer Arbeitsgruppe. Am Schluss werden die Bucheinträge von ihnen und ihrem Gruppenmitglied **zusammengezählt**. Jeder Bucheintrag dieser Summe wird mit **€0.20** vergütet. Der Gesamtbetrag wird anschließend **gleichmäßig** auf beide Gruppenmitglieder **aufgeteilt**.

Ihr Einkommen setzt sich wie folgt zusammen:

Fester Stundenlohn

€3.50/Std · 4 Std = €14.00

+

Leistungsabhängiger Lohn

Alle Bucheinträge Ihrer Arbeitsgruppe	Ihre Anzahl Bucheinträge + Anzahl Bucheinträge Ihres Gruppenmitglieds
Leistungslohn Ihrer Arbeitsgruppe	Alle Bucheinträge Ihrer Arbeitsgruppe · €0.20
Ihr Leistungslohn	Leistungslohn Ihrer Arbeitsgruppe

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